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# EDMUND CLARK SANFORD 1859-1924

Acting Editor 1888-9; Associate Editor 1895-1920; Cooperating Editor 1920-4

Dr. Edmund C. Sanford, head of the department of psychology in Clark University, died suddenly in Boston, November 22, 1924. His loss will be felt nationally and institutionally, as well as personally. He was one of the acknowledged leaders of the second generation of American psychologists. He spent his whole professional life—with the exception of a year's instructorship at Johns Hopkins—in the service of Clark University and Clark College. In social intercourse he did not easily let down the bars of an unvarying and somewhat formal courtesy; but those who were privileged to know him intimately know that he had a genius for friendship.

Edmund Clark Sanford was born in Oakland, California, November 10, 1859. Both his father and his mother came of old New England stock. The Sanfords were among the early settlers of Connecticut; the line includes a colonial governor of that State; and a certain David Sanford served as chaplain in the revolutionary army. The Clarks, too, were an original Connecticut family; a Clark of the sixth generation behind Sanford was one of the founders of Farmingham. His grandmother, Milicent Washburn Clark, carried her pedigree back to Plymouth Rock and 1620; Thomas Welles, governor of Connecticut in 1655, was one of her ancestors. It happened, however,—I do not know the circumstances,—that Edmund Sanford and Jennie Clark met and married in California, where their son was born.

Sanford was educated in the public schools of Oakland, whose citizens were largely of New England descent and took a characteristic pride in their school system. From the High School he passed, in 1879, to the university of California, then a private institution of the same town. His father, a druggist, died in February of the following year, and Sanford was obliged to leave the university for a time and to devote himself to business. As soon as he could be spared, he returned to his academic work, and graduated with the A.B. degree in May, 1883.

While he was in charge of the drug-store Sanford had decided upon the study of medicine; he "would rather write prescriptions than put them up!" He needed money for his medical preparation; and, as nothing better offered itself, he accepted a position as teacher in the neighboring village of Trout Brook Cañon, to and from which he rode daily. Almost immediately, however, he received an invitation (probably through the employment secretary of the university) to go for two years to Oahu College, Honolulu, as teacher of "the Classics, Mathematics (Algebra, Geometry, Trigonometry, Surveying and Navigation) and Ancient History." He had the courage to accept, and sailed off in September, 1883, taking his medical books with him. I know nothing of his time in Honolulu except that he spent the summer of 1884 as a guest in the house of some pupils and had the opportunity to make the famous 'volcano trip' to Kilauea. The two years proved to him, however, that his real interest lay in teaching; he gave up the idea of medicine, and returned to California in June, 1885, bent upon an educational career.

In the autumn of the same year he entered the Johns Hopkins University as a graduate student. It was natural for him to turn to Baltimore; for not only was the Hopkins practically the only university in the country offering graduate work, but Sanford's cousin, Charles Howard Shinn, was also a Hopkins man; the change from medicine to education was, therefore, in all probability, nothing more than a change from department to department within the chosen university. Education, however, meant Stanley Hall, and the Stanley Hall of that period was busied with experimental psychology as well as with education. It was, undoubtedly, Hall's influence that led San-

ford to adopt psychology as his major interest; but I know no details. He stayed at the Hopkins for three years as student, receiving a scholarship in 1886 and a fellowship in 1887; he graduated with the Ph.D. in 1888; and he remained for a fourth year as instructor in psychology.[1]

During this year, 1888-9, he was also on the books of Clark University; Stanley Hall was in Europe, and Sanford edited the JOURNAL in his absence. He went to Clark, as instructor in psychology, in July, 1889, and maintained an unbroken connection with the University and College to his death in 1924. In 1892 he was made assistant professor of psychology, with exclusive charge of the laboratory; in 1900 he was promoted to the professorship of experimental and comparative psychology. In 1909 he became the second president (following Carroll Davidson Wright) of Clark College.

Sanford did not turn, without mature consideration, from scientific to administrative and educational work; he realized that he was giving up a profession in which he had become a leader for another, equally arduous, in which he was untried. In seeking to understand his reasons, we must remember, first, that education had been his early love; he had 'found himself' during his two years in Honolulu. We must remember also—I return to the point later—that he was never robust, and that a man of fifty cannot meet the sheerly physical demands of laboratory work as a younger man can. I believe, however, that the real ground of the change lay in Sanford's attitude to psychology itself. No man of high intelligence can be content to produce experimental papers, year after year, without looking beyond them for some sanction, some coordinating principle that shall hold them all together. And experimental psychology, it seems to me, can offer only two such sanctions: system, the applied logic of general psychology, and application, the putting of psychological facts and laws to some social use. But Sanford, like his master Stanley Hall, was not a systematist; the problems of systematic psychology found him cold—even a little impatient. It was natural, then, that he should turn in middle life to

[1] I owe to Sanford's sister, Miss Martha L. Sanford, most of the information contained in these paragraphs. Certain details have come to me from Mrs. W. F. Frear, one of Sanford's pupils at Oahu College, and from Dr. L. N. Wilson, Librarian of Clark University.

some form of application; and the presidency of the College promised him an opportunity of applying his psychology that accorded admirably with his adolescent interests. He thought the matter over, and accepted the position.

It seemed, at the time, that the decision meant a final separation from psychology, and that Sanford was to spend the rest of his working life as administrator.[2] Circumstances ruled otherwise. In 1920 it became possible, for the first time under the provisions of the Founder's will, to bring Clark University and Clark College under a single administrative head. The Trustees approved the union, and Sanford resigned the presidency of the College. He was immediately appointed professor of psychology and education in the University, in succession to Stanley Hall, and he held this chair to the end. He spent the academic year 1920-21 in Baltimore, preparing for his new duties, and then took up again his professional work; he would have retired, for age, in February, 1925.

Sanford married, December 28, 1901, Florence Bartling, his class-mate in the Oakland High School and at the University of California. His wife died December 1, 1922.

He received honorary degrees from Hobart College (Sc. D., 1909), the University of California (LL. D., 1912), and Clark University (LL. D., 1924). He was president of the American Psychological Association in 1902.

Sanford was a man of one book—and he left that only half finished. He had planned to write another, that should deal with the educational ventures of Clark College from its establishment in 1902 to the end of the first decade of his own presidency. As late as 1923 he was asking my advice whether he should attempt this new work or should add the lacking Part II. to his *Laboratory Course*. So far as I know, he did not even make a beginning upon either. This seeming inertia calls for explanation, especially as there are other indications of the same sort. No one, I think, who knows Sanford's reputation as a psycholog-

[2]Sanford himself was so sure of his farewell to psychology that he distributed his professional library among those colleagues and former students who he thought would make good use of it; he had an exceptionally valuable collection of 'separates.' It was characteristic of him to give the books away instead of selling them; it is to the honor of the recipients that, when he resumed his chair, they offered with one accord to return them.

ist, can look over the Bibliography appended to this sketch without wondering why the list is not longer. And those of us who recall the early meetings of the Experimentalists remember also that Sanford had all sorts of plans, problems, ideas for apparatus and what not, that were formulated, discussed and then—dropped. There must have been a reason.

I feel sure that the reason may be stated, very simply, as precarious health, constitutional weakness. Sanford has more than once told me that he worked very slowly; and he gave an immense amount of time to his students; the contents of the papers presently to be listed show that with all clearness, even if one had no outside knowledge of the fact. But there can be little doubt that he was handicapped by bodily limitations. In his school-days he suffered two severe illnesses, typhoid fever and what was then called inflammation of the brain; and in his early years at Clark he underwent a similarly severe attack of rheumatic fever, which very probably affected his heart. The true wonder is, then, not that he did not publish more, but that he was able to accomplish as much as he did.[1]

The one book is, of course, an historic document. Six instalments, under the title A Laboratory Course in Physiological Psychology, were published in the JOURNAL between 1891 and 1896. The first four, recast into six chapters which covered the field of sensation, were issued in book-form ('edition of advanced sheets') in 1894, as A Course in Experimental Psychology. The early copies appeared in paper covers,—one of my professional treasures is the booklet in dingy blue that carries Sanford's autographed 'compliments,'—but these were soon exchanged for the familiar cloth of Heath's Pedagogical Library. Part I. was completed by the issue in 1898 of three more chapters: on the visual perception of space and motion (this chapter includes the last two of the JOURNAL instalments), on Weber's Law and the psychophysical methods, and on apparatus. The new chapters, with appendices on Listing's Law and the horopter, and with notes and suggestions on chapters I.-VI., were published both as

[1]Miss Sanford tells me that she remembers having heard her brother called 'lazy,'—surely a curious adjective to apply to a youth who, with Sanford's handicaps, had passed through High School and College in the normal period!

a separate book and also in combination with the 'advanced sheets' of these earlier chapters. The two portions of the single volume were not, however, brought down to the same date; chapters I.-VI. take us only to the end of 1893, chs. VII.-IX. to the end of 1896. Part. II was to have contained chapters on "Reflex and Voluntary Movement, The Time Relations of Mental Phenomena, Association, Memory, Attention and Emotion, so far as these subjects can be approached with experiments of moderate difficulty, together with a chapter on the apparatus necessary for such experiments."

The importance of this Laboratory Course, in one respect, may be gathered from the words of Sanford's first preface. "The author's excuse," he says, "for allowing the publication, even in this modest form, of so incomplete a work, must be the very extraordinary condition of experimental psychology at this time. Many laboratories have been opened, and many teachers of psychology are anxious to give their students the benefit of demonstrations and practice work, and yet there is absolutely no laboratory handbook of the subject to be had. At such a time half a loaf may be better than no bread." The 'very extraordinary' is strong, but not too strong. In the nineties of the last century psychological laboratories—laboratories in which psychology was supposed to be taught to undergraduate students—were springing up all over the continent, while as yet nobody knew what to teach or how to teach it. The *Einführungscursus* which some of us had taken in Leipzig covered only the metric methods and the technique of the chronoscope, and these were hardly subjects for the American sophomore. It is therefore no overstatement to say that Sanford's JOURNAL papers and the resulting book saved a rather critical situation; they furnished, so to speak, a common denominator for laboratory courses; they guided the uninformed and steadied the unstable. And since—as the Preface does not and could not tell us—the work of selection and presentation was excellently done, the book has a permanent as well as an historic value. I should like to repeat what I wrote of it in 1901, just ten years after the appearance of the first paper: "It has a high historical importance, as the first manual of experimental psychology; it has exerted, and still exerts, a wide influence, as the gateway through which American

students are introduced to laboratory work; and it is a monument of accurate erudition." Would that it had been brought to completion!

The list of Sanford's published writings, like those of the writings of many other men of science, begins with verse. The little poems are not intrinsically important; but the mention of them gives me the opportunity to refer to a second principal influence brought to bear on Sanford's thought and attitude,—an influence felt earlier than that of Stanley Hall and on the whole, perhaps, felt more deeply. In his undergraduate days Sanford came under the spell of Edward Rowland Sill;[4] and all his life long his judgment in matters of literary taste, his choice of books for holiday reading, his attitude to music and to the other arts, his whole aesthetic outlook were, with but little modification by personal temperament, the judgment and attitude and outlook of his former professor. One could almost always tell beforehand what his response would be to a given problem of aesthetics or to a given work of art; and once I was goaded to remark—he was fond of the word 'reaction'—that he need not on that account write himself down a reactionary. I used especially to argue with him that novel-reading is an essential part of a psychologist's education; but I could not convince him, and he stuck to his Biographies. He had it in him, I am sure, to acquire independence: but, after all, the gaining of an independent aesthetic judgment means very hard work, and I have already intimated that Sanford had no reserve of physical stamina upon which to draw. From this point of view, then, we may count it a stroke of good fortune that the influence which moulded his aesthetic life was as wholesome and as discriminating as Sill's must undoubtedly have been.

For the rest, the bibliography, besides showing Sanford's general competence in experimental and comparative psychology, bears witness to three special abilities that should not be left

[4]Miss Sanford has shown me a copy of *The University of California Magazine*, 2, 1896, no. 4: *Sill Memorial Number*, in which Sanford writes (134-136) of "Professor Sill as a Teacher." Sanford speaks of "the transcendent good fortune" of having come under Sill's instruction. He mentions a "Psychology Club, which met from time to time at Professor Sill's home in the evening," when topics of philosophy and ethics came under discussion. It is at least possible that this connection of 'psychology' with Sill prepared him to accept psychology when he met Stanley Hall.

without a word or two. First of all, he had a particular gift for popularisation; the lectures on the Function of the Several Senses in the Mental Life and on Psychic Research in the Animal Field are models of their kind; and there were many others of the same type and of like merit—I remember vividly a lecture on the Psychology of Sleight of Hand—which have not found their way into print. Secondly, he had what for want of a better term I must call the knack of the Minor Study. It is far easier now than it was thirty years ago to find experimental problems that shall occupy a student only for a single academic year and whose results shall still be worth publishing; but Sanford's first Minor Study appeared in 1893. Professor Jastrow and he were, indeed, the pioneers in a line of work that has proved both fruitful to science and valuable for education. And, thirdly, he had the love of the true laboratory man for making things with his own hands; I suppose that he was never happier than when 'playing in the shop.'[5] The Clark Laboratory of the older days had something of the attraction of a puzzle or a conundrum; one was always coming upon bits of apparatus whose use and meaning set one guessing. Many of these inventions, as I have said, were left unfinished; but many were completed and described, and a good number have become part of the regular equipment of our laboratories. One of the best known, the vernier chronoscope, has won international recognition; Sanford described a rough model in 1890, and the perfected forms in 1898 and 1901.

If, then, one might be tempted, without knowledge of Sanford's physical condition, to express surprise that the list of his psychological writings is not longer, one could have no least doubt of the quality of his output or of the mental calibre of the man behind it. America is justly proud of the exceptional three who make up the first generation of her psychologists; two of them, we all agree, showed marks of what may properly be called genius. But America has no reason to be anything else than proud of a second generation of which, in range, in originality and in thoroughness of work, Sanford should be a typical representative. E. B. T.

[5]Sanford was an excellent mechanic. He built, among other things, a telescope,—grinding and polishing the lenses himself,—which he recently made over to Professor F. B. Williams for use in the department of mathematics in Clark University.

## APPENDIX I

### Sanford's Work with Students

I remarked above that Sanford gave an immense amount of time to work with and for his students. That was the Clark policy and the Clark tradition,—that every member of the department should be accessible for consultation to every student; Hall and Sanford and Professor Burnham were thus equally at the student's disposal, whichever of the three might be ultimately responsible for publication. I feel, as his former students themselves will also feel, that any outline of Sanford's professional activity which left out a reference to this work—work both of direction and of participation—would be misleading and incomplete. I have therefore decided, as the most nearly objective procedure, to print a list of published articles in which we know that Sanford was in some way and in some degree concerned. The list will be a reminder to his old students of that intellectual partnership of two generations which is one of the delights of work of research; and his suggestions of subjects of investigation, since they indicate the direction of his own psychological interest, will help to complete the picture that the list of his published writings gives us.

*I. Theses signed by G. S. Hall and E. C. Sanford*[6]

1891. Nichols, H. The psychology of time, 3, 453-529; 4, 60-112.
"I was requested by the instructor in Psychology at Clark University to investigate . . . the Constant Error of Time-judgments." Sanford served as observer.

1892. Bryan, W. L. On the development of voluntary motor ability, 5, 125-204.
Sanford served as observer, and devised mechanical counter.

1894. Bergström, J. A. An experimental study of the conditions of mental activity, 6, 247-274.
"Chiefly done under the direction of" Sanford, who served as observer.

1894. Dresslar, F. B. Studies in the psychology of touch, 6, 313-368.

1897. Lindley, E. H. A study of puzzles with special reference to the psychology of mental adaptation, 8, 431-493.
"Suggestion of the topic; . . . advice and criticism in the working out of it."

*II. Theses signed by E. C. Sanford*

1896. Drew, F. Attention; experimental and critical, 7, 533-572.
"General direction and constant aid and counsel." Sanford served as observer, and added a Note reporting check experiments.

1900. Small, W. S. An experimental study of the mental processes of the rat, 11, 133-165; 12, 206-239.
"Initial suggestion, ample laboratory facilities, continued interest and helpful criticism."

1901. Kinnaman, A. J. A comparison of judgments for weights lifted with the hand and foot, 12, 240-263.

1902. Sears, C. H. A contribution to the psychology of rhythm, 13, 28-61.
"Suggestion [of topic]; . . . help rendered throughout."

1903. Swift, E. J. Studies in the psychology and physiology of learning, 14, 201-251.

1904. Kuhlmann, F. Experimental studies in mental deficiency, 15, 391-446.
"Suggestion [of topic]; . . . constant advice and many valuable suggestions."

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[6] For the headings and items of the first two lists I am indebted to Dr. L. N. Wilson. All references, unless otherwise specified, are to this Journal.

1906. Terman, L. M. Genius and stupidity, *Ped. Sem.*, 13, 307-373.
1906. Browne, C. E. The psychology of the simple arithmetical processes: a study of certain habits of attention and association, 17, 1-37.
1906. Porter, J. P. The habits, instincts, and mental powers of spiders, genera *argiope* and *epeira*, 17, 306-357.
Thanks Hall and Sanford "for suggesting the topic of this research and for assistance in studying it."
1906. Gesell, A. L. Accuracy in handwriting as related to school intelligence and sex, 17, 394-405.
1907. Cleveland, A. A. The psychology of chess and of learning to play it, 18, 269-308.
"Suggestion of this topic of study, and...generous assistance in following it out."
1907. Davis, H. B. The raccoon: a study in animal intelligence, 18, 447-489.
1908. Book, W. F. The psychology of skill: with special reference to its acquisition in typewriting, *U. of Montana Publications in Psych.*, 1, 1-188.
1909. Ellison, L. The acquisition of technical skill, *Ped. Sem.*, 16, 49-63.
1910. Rowe, E. C. Voluntary movement, 21, 513-562.
1911. Kakise, H. A preliminary experimental study of the conscious concomitants of understanding, 22, 14-64.
1923. Fryer, D. Intelligence and interest in vocational adjustment, *Ped Sem.*, 30, 127-151.
1924. Richards-Nash, A. A. The psychology of superior children, *Ped. Sem.*, 30, 209–246.

It is plain to a psychologist that these papers do not all stand on the same level; the thesis is a function of the student, as well as of his professor! Yet I think it would be difficult to find another list of eighteen consecutive theses that should show a larger proportion of solid work. Drew's article has held its place among the selected references of Ebbinghaus' *Psychologie* through three editions; Washburn attributes to Small priority in the use of the 'labyrinth method'; and many other items of the list have received like recognition.[7]

Several of the authors cited express their thanks to Sanford for 'criticism.' To one who did not know him well, the term will seem to accord ill with his usual gentle speech and with the tone of his printed reviews. He disliked, it is true, to say harsh things in print, and he disliked controversy. Man to man, however, in correspondence and in conversation, Sanford could command a very searching criticism,—all the more searching as it was invariably of the internal sort; he did not set up against yours an argument or a method of his own, but he looked to see what flaw there was in your method and your argument, and thus helped you to strengthen an attack or a position. Many, many times I have invited his criticism, and always I have profited by it. So his students, looking back over their struggles with procedure and with literary presentation, could hardly fail to realise—though the full realisation would come only later, when their own experience had ripened—something of what his critical comments and objections had done for their work.

*III. Other Articles in this Journal*[8]

1891. Fraser, A. Visualisation as a chief source of the psychology of Hobbes, Locke, Berkeley, and Hume, 4, 230-247.
"The starting-point of this paper was a suggestion [from Sanford]... that I should investigate the figures of speech used in psychology."

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[7] H. Ebbinghaus, *Grundzüge der Psychologie*, 1, 1902, 595; 1905, 621; 1911, 780. M. F. Washburn, *The Animal Mind*, 1908, 219.

[8] This list has been hurriedly prepared, and I am not sure that it is complete.

1894. Bolton, T. L. Rhythm, 6, 145-238.
Ascribes to Sanford "much of the credit for the success of the work. But for his skill in devising and constructing apparatus, the work could not have been carried on. His suggestions as regards methods for making the experiment were no less valuable than his assistance in devising apparatus."
1894. Bergström, J. A. The relation of the interference to the practice effect of an association, 6, 433-442.
1896. Scott, C. A. On old age and death, 8, 67-122.
1896. Smith, T. L. On muscular memory, 7, 453-490.
Sanford observed, and devised automatic shutter.
1896. Lindley, E. H. A preliminary study of some of the motor phenomena of mental effort, 7, 491-517.
1897. Phillips, D. E. Genesis of number-forms, 8, 506-527.
1898. Kline, L. W. The migratory impulse *vs.* love of home, 10, 1-81.
1899. Colegrove, F. W. Individual memories, 10, 228-255.
1899. Kline, L. W. Methods in animal psychology, 10, 256-279.
Thanks for "suggesting the work itself"; and for "valuable help and timely suggestions at every turn during its execution."
1899. Kline, L. W. Suggestions toward a laboratory course in comparative psychology, 10, 399-430.
Thanks for "the original plan of the work, and for seeing that ample laboratory material was provided me, together with much assistance in the arrangement of the subject-matter of this paper."
1899. Goddard, H. H. The effects of mind on body as evidenced by faith cures, 10, 431-502.
1900. Dawson, G. E. Psychic rudiments and morality, 11, 181-224.
1900. Triplett, N. The psychology of conjuring deceptions, 11, 439-510.
1902. Kinnaman, A. J. Mental life of 2 Macacus Rhesus monkeys in captivity, 13, 98-148; 173-218.
1902. France, C. J. The gambling impluse, 13, 364-407.
1902. Slaughter, J. W. A preliminary study of the behavior of mental images, 13, 526-549.
"Undertaken at the suggestion of Dr. Sanford and part of the work . . . carried on with his assistance."
1904. Porter, J. P. A preliminary study of the psychology of the English sparrow, 15, 313-344.
1904. Gault, R. E. A sketch of the history of reflex action in the latter half of the nineteenth century, 15, 526-568.
"On whose [Sanford's] suggestion this work was undertaken and by whose counsel it was carried to completion."
1906. Porter, J. P. Further study of the English sparrow and other birds, 17, 248-271.
1907. Kuhlmann, F. On the analysis of the memory consciousness for pictures of familiar objects, 18, 389-420.
1909. Waddle, C. W. Miracles of healing, 20, 219-268.
1909. Guillet, C. Retentiveness in child and adult, 20, 318-352.

## APPENDIX II

### Bibliography of the Published Writings of Edmund Clark Sanford

Compiled by Edith M. Baker, Assistant Librarian, Clark University

1. Her 'broidery work. *Overland Monthly*, 2d ser., 1, 1883, 110.
2. For a plaque. *Ibid., loc. cit.*
3. A belated butterfly. *Ibid.*, 1, 1883, 211.
4. A pasteboard Cupid. *Ibid., loc. cit.*

5. Fate. *Ibid.*, 2, 1883, 223.
6. Song. *Ibid.*, 4, 1884, 334.
7. Poppies and grass-flowers. *Ibid.*, 4, 1884, 520.
8. Mid-ocean. *Ibid.*, 5, 1885, 212.
9. Song. *Ibid.*, 6, 1885, 601.
10. Writings of Laura Bridgman; with an introductory note by G. S. Hall. 1887, pp. 34. Reprint from *Overland Monthly*, 8, 1886, 355-373, 577-586.
11. Relative legibility of the small letters. This JOURNAL, 1, 1888, 402-435.
12. Personal equation. *Ibid.*, 2, 1888, 1-38; 1889, 271-298; 403-430.
13. A simple and inexpensive chronoscope. *Ibid.*, 3, 1890, 174-181.
14. Psychology at Clark University. *Ibid.*, 3, 1890, 284-285.
15. A laboratory course in physiological psychology. *Ibid.* (1) Introduction; dermal senses; static and kinaesthetic senses, 4, 1891, 141-155; (2) Taste and smell, 4, 1891, 303-322; (3) Vision, 4, 1892, 474-490; (4) Vision, 5, 1893, 390-415; (5) Visual perception of space, 6, 1895, 593-616; (6) Monocular perception of space, 7, 1896, 412-424.
16. Notes on studies of the language of children. *Ped. Sem.*, 1, 1891, 257-260.
17. Possibility of a realization of four-fold space. *Science*, 19, 1892, 332.
18. New visual illusion. *Ibid.*, 21, 1893, 92-93.
19. (and Reigart, J. F.) On reaction times when the stimulus is applied to the reacting hand. This JOURNAL, 5, 1893, 351-355.
20. A new pendulum chronograph. *Ibid.*, 5, 1893, 385-389.
21. Some practical suggestions on the equipment of a psychological laboratory. *Ibid.*, 5, 1893, 429-438.
22. A course in experimental psychology. Boston, Heath & Co., 1894, pp. 183. (Cf. 15.)
23. Notes on new apparatus. This JOURNAL, 6, 1895, 575-584.
24. The Philadelphia meeting of the American Psychological Association. *Science*, 1896, N. S., 3, 119-124.
25. Proc. of the 4th annual meeting of the American Psychological Association, 1895. Report of the secretary and treasurer for 1895. *Psy. Rev.*, 3, 1896, 121-123.
26. Course in experimental psychology; pt. 1: sensation and perception. Boston, Heath & Co., 1898, pp. 449. (Cf. 15, 22.)
27. The vernier chronoscope. This JOURNAL, 9, 1898, 191-197.
28. Cours de psychologie expérimentale (sensations et perceptions); tr. de l'Anglais par A. Schinz; rev. par M. Bourdon. Paris, Schleicher Frères, 1900, pp. 477. (Bibl. de péd. et de psychol.)
29. Studies of rhythm and meter. This JOURNAL, 12, 1901, 361-389.
30. Improvements in the vernier chronoscope. *Ibid.*, 12, 1901, 590-594.
31. Illustrations of the application of psychological principles to ethical problems. *Ped. Sem.*, 9, 1902, 18-27.
32. Mental growth and decay. This JOURNAL, 13, 1902, 426-449.
33. Psychology and physics. *Psy. Rev.*, 10, 1903, 105-119.
34. The psychic life of fishes. *Internat. Quarterly*, 7, 1903, 316-333.
35. On the guessing of numbers. This JOURNAL, 14, 1903, 647-665.
36. Responsiveness to beauty. *Clark Univ. Library. Pubs.*, 1, 1905, 223-228.
37. Sketch of a beginner's course in psychology. *Psy. Bull.*, 3, 1906, 59-60. (Abstract.)
38. Sketch of a beginner's course in psychology. *Ped. Sem.*, 13, 1906, 118-124.
39. (and T. J. Stevenson) A preliminary report of experiments on time relations in binocular vision. This JOURNAL, 19, 1908, 130-137.
40. Inaugural address. *Clark Coll. Rec.*, 5, 1910, 107-119.

41. The teaching of elementary psychology in colleges and universities with laboratories. *Psy. Monographs*, 12, no. 4, 1910, 54-71.
42. The address of welcome, commencement 1910. *Clark Coll. Rec.*, 5, 1910, 215-217.
43. Experimental pedagogy and experimental psychology. *Jour. of Educ. Psy.*, 1, 1910, 590-595.
44. Address of welcome, commencement 1911. *Clark Coll. Rec.*, 6, 1911, 155-160.
45. Function of the several senses in the mental life. This JOURNAL, 23, 1912, 59-74.
46. Methods of research in education. *Jour. of Educ. Psy.*, 3, 1912, 303-315.
47. Address of welcome, with review of the first ten years of Clark College, commencement 1912. *Clark Coll. Rec.*, 7, 1912, 144-152.
48. Entrance requirements and the college degree. *Educ.*, 33, 1913, 281-288. Also as: Admission requirements. *Clark Coll. Rec.*, 8, 1913, 101-110.
49. The address of welcome, Clark College commencement, June 1913. *Ibid.*, 8, 1913, 164-170.
50. Psychic research in the animal field. Der kluge Hans and the Elberfeld horses. This JOURNAL, 25, 1914, 1-31.
51. Address of welcome, commencement, June 1914. *Clark Coll. Rec.*, 9, 1914, 189-193.
52. Recollections of student life in a western university. *Clark Coll. Mo.*, 4, 1914, 101-109.
53. Address of welcome, commencement 1915. *Clark Coll. Rec.*, 10, 1915, 183-187.
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# THE EFFECT OF VARYING THE INTENSITY OF LIGHT ON THE DISAGREEMENT OF FLICKER AND EQUALITY-OF-BRIGHTNESS PHOTOMETRY FOR LIGHTS OF DIFFERENT COMPOSITION

By C. E. Ferree and Gertrude Rand, Bryn Mawr College

In two previous papers[1], the cause of the disagreement of the flicker and equality-of-brightness methods of photometry was discussed, and the relation of the lag in visual sensation to this disagreement was experimentally shown. It is the purpose of this and the following papers to develop farther the argument advanced in those papers, by showing that the effect of varying the intensity of light and the speed of rotation of the exposure disc on the disagreement of the results obtained by these two methods of photometry is just what would be expected from lag as the cause of the disagreement.

In the series of experiments planned to carry out the comparison between the results by the methods of flicker and equality-of-brightness on the one hand, and the rise of sensation on the other, three intensities of light were used, 12.5, 25 and 50 meter-candles. Several intensities were added during the course of the work in order to investigate certain points that came out in the study of the relation of the two photometric methods. The general trend of the results obtained in these experiments was checked and verified by 20 *O*s.

It was found that the type of disagreement between the two methods was different for each of these three intensities. For example, at 12.5 m.c. B was overestimated by 10.23%; and R, Y and G were underestimated by 22.54, 21.26 and 4.72% respectively. At 25 m.c. R, Y, G and B were all underestimated,—R by 6.56%, Y by 3.96%, G by 17.15% and B by 24.28%. At 50 m.c. R was overestimated by 17.89% and Y by 23.83%. B and G were underestimated,—B by 16.67% and G by 15.74%. At intensities somewhat lower than 12.5 m.c., *e. g.*, at 10.20 m.c., B and G were overestimated and R and Y were underestimated. That is, between these lower intensities and intensities in the neighborhood of 50 m.c. there was a complete reversal of the type of disagreement for the R and Y and for the B and G. Figg. 1-4 should be consulted for a compari cn

[1]The Cause of the Disagreement Between Flicker and Equality-of-Brightness Photometry, this Journal, 35, 1924, 190-208; and Flicker Photometry and the Lag of Visual Sensation, *ibid.*, 35, 1924, 209-216.

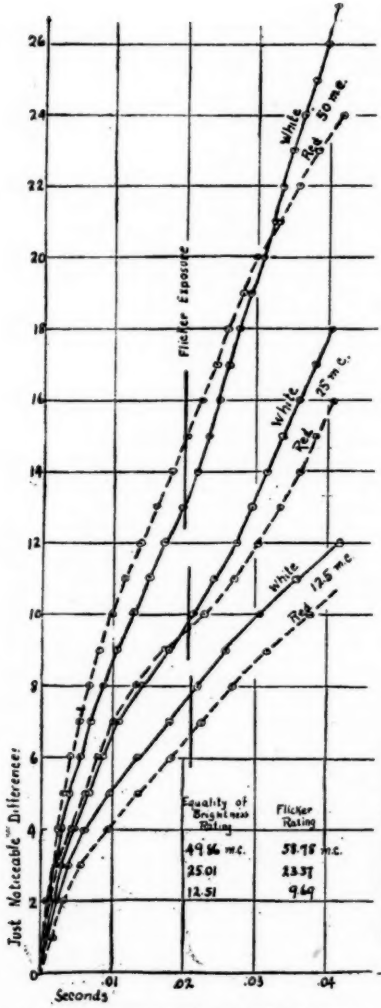


FIG. 1. Curves showing the rise of sensation up to 0.04 sec. exposure for red and white, 12.5, 25 and 50 meter-candles

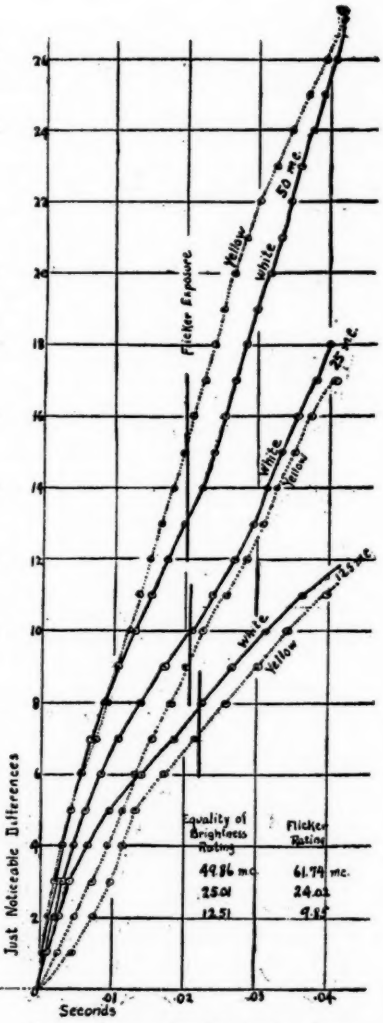


FIG. 2. Curves showing the rise of sensation up to 0.04 sec. exposure for yellow and white, 12.5, 25 and 50 meter-candles

of the rise-of-sensation curves with the amount and direction of these deviations from those obtained by the equality-of-brightness method.

Since all of the colors showed the characteristic shift from underestimation to overestimation or the converse, it seemed reasonable to expect that an intensity might be found for each pair of lights at which agreement with the equality-of-brightness rating would occur. The search for this intensity was somewhat of a trial-and-error character,—controlled, however, by a fairly regular decrease in the disagreement as the intensity in question was approached. For example, R was overestimated at 50 m.c.; underestimated at 25 m.c.; and still more underestimated at 12.5 m.c. Agreement was found at 29.39 m.c. A similar indication of the region of intensity in which agreement might be expected to occur was had in case of each of the other colors. Agreement was found for Y at 26.12 m.c.; for G at 11.91 m.c.; and for B at 14.91 m.c. The region over which agreement could be obtained was narrow, as is shown by the large angle at which the curve expressing percentage disagreement of the results by the method of flicker crosses the line representing agreement for the different intensities as rated by the equality-of-brightness method (Fig. 6). The three remaining colors were photometered at each of the intensities at which agreement was found for one of the colors, in order to get some idea of the deviation that might be expected for the other parts of the spectrum if one of these intensities were selected as standard. For example, at 14.91 m.c. (intensity at which agreement occurred for B) R was underestimated by 14.69%; Y by 17.71%; and G by 8.79%. At 11.91 m.c. (intensity at which agreement occurred for G) R was underestimated by 21.92%; and Y by 21.25%. B was overestimated by 10.83%. These determinations were made at the speed of rotation of the disc ordinarily used in the method of flicker, *i. e.*, the speed which gives the greatest sensitivity. When the speed was changed, agreement no longer occurred at these intensities. The amounts of disagreement for the other colors were also shifted by the change of speed. It is quite possible, of course, that another intensity may be found for each pair of lights above 50 m.c. or below 12.5 m.c. at which agreement would occur.

In searching for the intensity at which agreement occurs in case of B, an interesting and very baffling phenomenon of low flicker sensitivity was found over a range on either side of the point at which agreement occurred with the equality-of-brightness method. A change from 13.6 to 15.8 m.c., as rated by the equality-of-brightness method, gave no difference in rating by the method of flicker. This low sensitivity by the method of flicker was very hard to understand until the rise-of-sensation

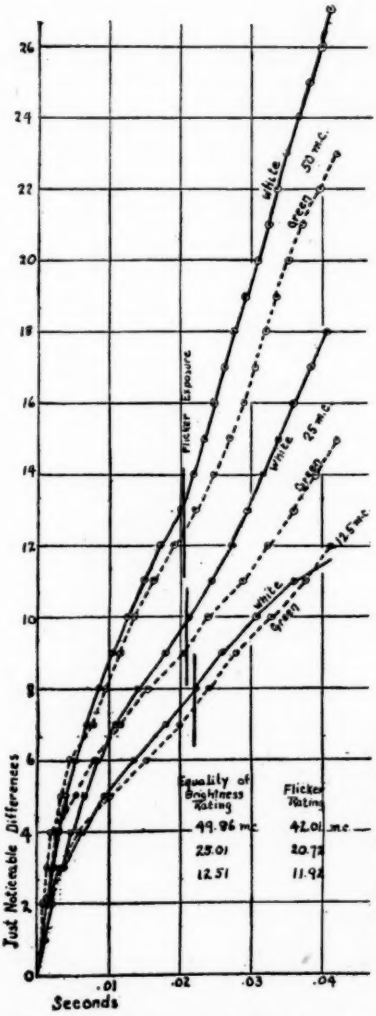


FIG. 3. Curves showing the rise of sensation up to 0.04 sec. exposure for green and white, 12.5, 25 and 50 meter-candles

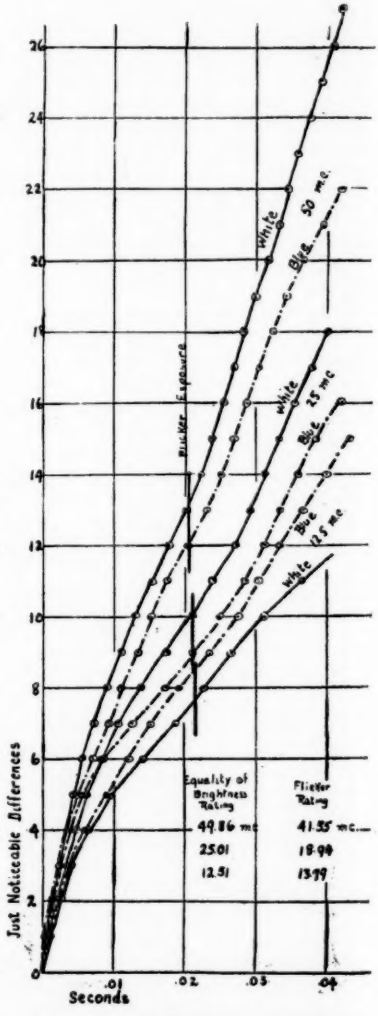


FIG. 4. Curves showing the rise of sensation up to 0.04 sec. exposure for blue and white, 12.5, 25 and 50 meter-candles

work for 12.5 and 25 m.c. was completed and the curves were plotted. A comparison of these curves showed very little difference in the sensation level for the lengths of exposure used in the method of flicker even for a change as great as from 12.5 to 25 m.c. That is, the rise-of-sensation curves for these two intensities of B lie very close together for quite a little distance above and below the point representing the flicker exposure. We were confronted here, then, by the somewhat anomalous result that, when the carbon standard was set to give 14.91 m.c. on the photometric field, the colored light could be changed through a range from 131.6 to 15.8 m.c., as rated by the equality-of-bright-

TABLE I

Showing the Effect of Intensity of Light on the Amount and Type of Disagreement of the Results by the Equality-of-Brightness and Flicker Methods

| Color | Photometric Rating | | Difference between the two methods | | Revolutions per sec. Flicker Disc. | Length of Individual Exposures |
|---|---|---|---|---|---|---|
| | Equality-of-Brightness Method | Flicker Method | | | | |
| | m.c. | m.c. | m.c. | % | | sec. |
| R | 49.86 | 58.78 | + 8.92 | +17.89 | 12.5 | 0.020 |
| (675mμ) | 34.03 | 35.56 | + 1.53 | + 4.50 | 12.4 | 0.02016 |
| | 29.39 | 29.39 | 0 | 0 | 12.2 | 0.02049 |
| | 26.12 | 24.46 | − 1.66 | − 6.36 | 12.2 | 0.02049 |
| | 25.01 | 23.37 | − 1.64 | − 6.56 | 12.15 | 0.02058 |
| | 14.91 | 12.72 | − 2.19 | −14.69 | 11.8 | 0.02119 |
| | 12.51 | 9.69 | − 2.82 | −22.54 | 12.0 | 0.02083 |
| | 11.91 | 9.30 | − 2.61 | −21.92 | 11.75 | 0.02128 |
| Y | 49.86 | 61.74 | +11.88 | +23.83 | 12.6 | 0.01984 |
| (579mμ) | 29.39 | 32.87 | + 3.48 | +11.84 | 12.2 | 0.02049 |
| | 26.12 | 26.12 | 0 | 0 | 11.9 | 0.02101 |
| | 25.01 | 24.02 | − 0.99 | − 3.96 | 12.2 | 0.02049 |
| | 14.91 | 12.27 | − 2.64 | −17.71 | 11.5 | 0.02174 |
| | 12.51 | 9.85 | − 2.66 | −21.26 | 11.35 | 0.02203 |
| | 11.91 | 9.38 | − 2.53 | −21.25 | 11.1 | 0.02252 |
| G | 49.86 | 42.01 | − 7.85 | −15.74 | 12.3 | 0.02033 |
| (515mμ) | 29.39 | 23.80 | − 5.59 | −19.02 | 12.4 | 0.02016 |
| | 26.12 | 21.71 | − 4.41 | −16.89 | 11.9 | 0.02101 |
| | 25.01 | 20.72 | − 4.29 | −17.15 | 11.8 | 0.02119 |
| | 14.91 | 13.60 | − 1.31 | − 8.79 | 11.5 | 0.02174 |
| | 12.51 | 11.92 | − 0.59 | − 4.72 | 11.3 | 0.02212 |
| | 11.91 | 11.91 | 0 | 0 | 11.2 | 0.02232 |
| | 10.20 | 11.18 | + 0.98 | + 9.61 | 11.0 | 0.02273 |
| B | 49.86 | 41.55 | − 8.31 | −16.67 | 12.3 | 0.02033 |
| (466 mμ) | 29.39 | 22.55 | − 6.84 | −23.27 | 12.0 | 0.02083 |
| | 26.12 | 20.00 | − 6.12 | −23.43 | 12.2 | 0.02049 |
| | 25.01 | 18.94 | − 6.07 | −24.28 | 12.0 | 0.02083 |
| | 15.80 | 14.91 | − 0.89 | − 5.63 | 12.0 | 0.02083 |
| | 14.91 | 14.91 | 0 | 0 | 12.0 | 0.02083 |
| | 13.60 | 14.91 | + 1.31 | + 9.63 | 12.0 | 0.02083 |
| | 12.51 | 13.79 | + 1.28 | +10.23 | 11.6 | 0.02155 |
| | 11.91 | 13.20 | + 1.29 | +10.83 | 11.8 | 0.02119 |

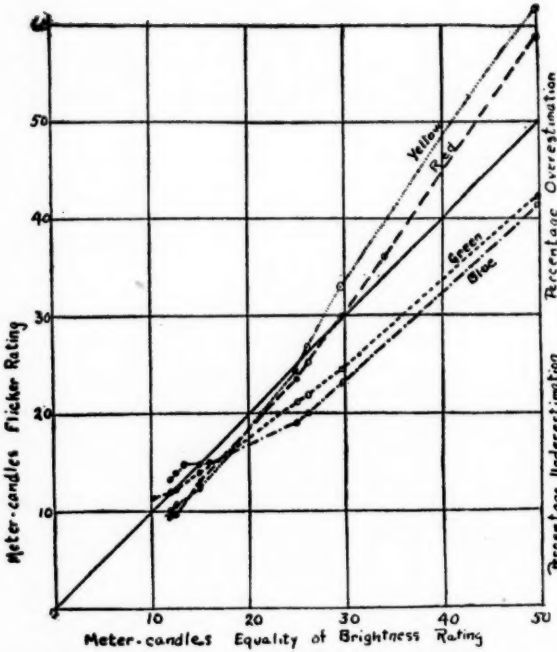


FIG. 5. Curves showing the effect of varying intensity on the disagreement between the equality-of-brightness and flicker ratings for lights of different composition

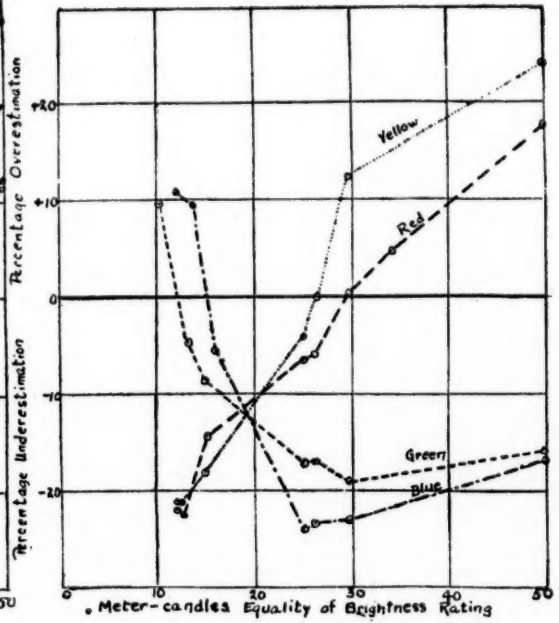


FIG. 6. Showing in percent. the overestimation and underestimation of the flicker as compared with the equality-of-brightness ratings for lights of different compositions at different intensities

ness method, without producing any change of result by the flicker method; yet the result of the comparison with the equality-of-brightness method had to be pronounced an underestimation of 9.63% by the method of flicker at the lowest point of this range; agreement at 14.91 m.c.; and an overestimation of 5.63% at the highest point in the range. As the sensitivity of the equality-of-brightness method was of the order of 1.5 to 2.5% we have here the unusual phenomenon of the equality-of-brightness method showing much greater sensitivity than the method of flicker.

The results of this work on the effect of variation of intensity are shown in Table I and Figg. 5 and 6. In Fig. 5 equality-of-brightness rating is plotted along the horizontal coordinate and flicker rating along the vertical coordinate. Agreement between the two methods is represented by a solid line bisecting the angle formed by the two coordinates. Points on the curve for each of the colors falling above the line show a higher rating by the flicker than by the equality-of-brightness method or overestimation by the method of flicker, and points falling below the line a lower rating by the flicker than by the equality-of-brightness method or underestimation by the method of flicker.

Fig. 6 represents percentage overestimation and underestimation by the method of flicker. The rating in m. c. by the equality-of-brightness method is plotted along the horizontal coordinate and percentage disagreement along the vertical. The zero line parallel to the horizontal coordinate represents agreement between the two methods. Points above and below this line represent respectively percentage overestimation and underestimation by the method of flicker at the different m.c. values. Agreement between the two methods occurs at the points where the flicker curve crosses the zero line.

# THE EFFECT OF SPEED OF ROTATION OF THE DISC ON THE DISAGREEMENT OF FLICKER AND EQUALITY-OF-BRIGHTNESS PHOTOMETRY FOR LIGHTS OF DIFFERENT COMPOSITION AND INTENSITY

By C. E. Ferree and Gertrude Rand, Bryn Mawr College

It has been shown in previous papers[1] that time of exposure is the chief cause of the disagreement between the results obtained by the flicker and equality-of-brightness methods of photometry. If time of exposure is a factor in the disagreement between these two methods, it would seem altogether probable that speed of rotation of the exposure disc would affect the amount and perhaps the type of disagreement. The effect had been noticed in the work in our laboratory long before it was made the subject of systematic investigation. In fact in the present series of Studies this was our first point of attack on the problem.

In making an investigation of this point, a means of producing very small changes of speed of rotation of the disc is required. For this purpose we were provided with a motor specially wound for speed control (500 to 6000 rev. per min.) and a rheostat of the ordinary sliding contact type connected in series with a drum which could be rotated, wound with many turns of low resistance wire. The speed of rotation of the disc was automatically recorded by a small speed-counter attached to the axle of the motor. The general trend of the results obtained was, as in the former Study, checked and verified by 20 *O*s.

For each of the 7 intensities of light mentioned in the preceding paper, the following speeds were used: the speed giving the greatest sensitivity to the method of flicker, one speed lower, and a number of speeds higher than this. For the most sensitive speed and the speeds higher than this, a position or a small range of positions could be found for the standard lamp which gave no flicker; for the lower speed, however, the balance had to be made

[1]The Cause of the Disagreement Between Flicker and Equality-of-Brightness Photometry, this Journal, 35, 1924, 190-208; Flicker Photometry and the Lag of Visual Sensation, *ibid.*, 209-216; The Effect of Varying the Intensity of Light on the Disagreement of Flicker and Equality-of-Brightness Photometry for Lights of Different Composition, *supra*.

in terms of minimum flicker. The sensitivity of the determinations varied considerably, of course, with change of speed. For the most sensitive speed it ranged from 0.20 to 0.68%; for the lower speed from 0.53 to 2.59%; and for the higher speeds from 0.64 to 25.68%.

The results showed consistently: (1) that the rating by the method of flicker varies with the speed of rotation of the disc; and (2) that the flicker and equality-of-brightness ratings were in some cases in closest agreement when the speed of rotation of the disc was low; in others when it was high. The result would obviously depend upon the relation of the rate of rise of sensation for the two lights above and below the point from which the change of speed was made. In much the greater number of cases studied, it will be noted that the closest agreement came with the lowest speed and the longest exposure. If we consider the trend of results very broadly, it may be said that, as the speed is decreased and the length of exposure is increased, agreement is approached irregularly as a limit, the limiting value of exposure being that used in the equality-of-brightness method. The results showed also (3) that the speed which gives the method of flicker its greatest sensitivity does not give the closest agreement in result with the equality-of-brightness method.

The changes in result are quite striking. The details are given in Tables I-III and in Figg. 1-4. In these charts metercandles as rated by the flicker method are plotted along the vertical coordinate and the length of exposure used, also cycles of the flicker disc per sec., along the horizontal coordinate. For comparison the equality-of-brightness ratings for the different intensities are indicated on the same m.c. scale at the right of the chart. A few of the results may be singled out here. A change in the speed from 11.8 to 14.1 cycles per sec. (a change in the length of the individual exposures from 0.02119 to 0.01773 sec.) for R at 50 m.c. changed an overestimation by the method of flicker from 8.08 to 18.31 m.c., a percentage change from 16.21 to 36.72. A change in speed from 12 to 16.2 cycles per sec. (a change in the exposure from 0.02083 to 0.01543 sec.) for B at 50 m.c. changed an underestimation of 7.34 to 12.67 m.c., a percentage change of from 14.72 to 25.41. A change in speed from 11.9 to 14.8 cycles per sec. (a change in the exposure from 0.2101 to 0.01689 sec.) for G at 50 c.m. changed an underestimation from 6.50 to 12.67 m.c., a percentage change of 13.04 to 25.41. And a change of speed from 11.9 to 14.4 cycles per sec. (a change in the exposure from 0.02101 to 0.01736 sec.) for Y at 50 m.c. changed an overestimation of 9.76 to 21.56 m.c., a percentage change of 19.57 to 43.24.

TABLE I

Showing the Effect of Speed of Rotation of the Disc on the Amount and Type of Disagreement of the Results by the Equality-of-Brightness and Flicker Methods

Color: R (675$m\mu$)

| Photometric Rating Equality-of-Brightness Method | Rev. per Sec. Flicker Disc | Length of Individual Exposures | Photometric Rating Flicker Method | Difference between the Two Methods | Sensitivity Flicker Method | Change Produced by Variation of Speed | |
|---|---|---|---|---|---|---|---|
| m.c. | | sec. | m.c. | % | % | m.c. | % |
| 49.86 | 11.8 | 0.02119 | 57.94 | +16.21 | 2.23 | 10.23 | 17.66 |
| | 12.5 | 0.020 | 58.78 | +17.89 | 0.58 | | |
| | 12.9 | 0.01938 | 60.06 | +20.46 | 2.13 | | |
| | 13.4 | 0.01866 | 62.28 | +24.91 | 4.28 | | |
| | 14.1 | 0.01773 | 68.17 | +36.72 | 11.29 | | |
| 34.03 | 11.6 | 0.02155 | 35.16 | + 3.32 | 2.16 | 5.17 | 14.70 |
| | 12.4 | 0.02016 | 35.56 | + 4.50 | 0.67 | | |
| | 12.9 | 0.01938 | 35.95 | + 5.64 | 2.20 | | |
| | 13.4 | 0.01866 | 37.62 | +10.55 | 5.48 | | |
| | 14.5 | 0.01724 | 40.33 | +18.51 | 16.62 | | |
| 29.39 | 11.4 | 0.02193 | 29.39 | 0 | 2.01 | 1.54 | 5.24 |
| | 12.2 | 0.02049 | 29.39 | 0 | 0.61 | | |
| | 13.0 | 0.01923 | 29.68 | + 0.98 | 2.97 | | |
| | 13.3 | 0.01880 | 30.30 | + 3.09 | 8.65 | | |
| | 13.6 | 0.01838 | 30.61 | + 4.15 | 11.31 | | |
| | 14.2 | 0.01761 | 30.93 | + 5.24 | 13.89 | | |
| 26.12 | 11.6 | 0.02155 | 23.80 | − 8.89 | 1.81 | 5.62 | 23.61 |
| | 11.9 | 0.02101 | 24.24 | − 7.20 | 1.44 | | |
| | 12.2 | 0.02049 | 24.46 | − 6.36 | 0.53 | | |
| | 12.9 | 0.01938 | 27.12 | − 3.83 | 2.39 | | |
| | 13.8 | 0.01812 | 28.12 | + 7.66 | 6.37 | | |
| | 14.3 | 0.01748 | 29.42 | +12.63 | 7.38 | | |
| 25.01 | 11.8 | 0.02119 | 22.36 | −10.60 | 2.59 | 7.63 | 34.12 |
| | 12.15 | 0.02058 | 23.37 | − 6.56 | 0.68 | | |
| | 12.4 | 0.02016 | 25.39 | + 1.52 | 2.76 | | |
| | 12.9 | 0.01938 | 26.63 | + 6.48 | 2.82 | | |
| | 13.3 | 0.01880 | 26.89 | + 7.52 | 4.69 | | |
| | 13.8 | 0.01812 | 27.68 | +10.68 | 8.27 | | |
| | 14.0 | 0.01786 | 28.01 | +11.91 | 8.46 | | |
| | 15.0 | 0.01667 | 29.99 | +19.92 | 9.46 | | |
| 14.91 | 11.4 | 0.02193 | 13.15 | −11.81 | 0.53 | 1.61 | 13.95 |
| | 11.8 | 0.02119 | 12.72 | −14.69 | 0.39 | | |
| | 12.4 | 0.02016 | 12.55 | −15.84 | 0.64 | | |
| | 13.2 | 0.01894 | 12.22 | −18.05 | 4.42 | | |
| | 13.9 | 0.01799 | 11.76 | −21.14 | 4.84 | | |
| | 14.8 | 0.01689 | 11.54 | −22.61 | 6.07 | | |
| 12.51 | 11.2 | 0.02232 | 10.40 | −16.86 | 1.83 | 1.21 | 13.12 |
| | 12.0 | 0.02083 | 9.69 | −22.54 | 0.20 | | |
| | 12.7 | 0.01969 | 9.46 | −24.37 | 4.45 | | |
| | 13.5 | 0.01852 | 9.30 | −25.65 | 6.55 | | |
| | 14.0 | 0.01786 | 9.19 | −26.53 | 20.06 | | |
| 11.91 | 11.4 | 0.02193 | 9.34 | −21.59 | 0.64 | 0.83 | 9.72 |
| | 11.75 | 0.02128 | 9.30 | −21.92 | 0.22 | | |
| | 12.3 | 0.02033 | 9.11 | −23.52 | 0.88 | | |
| | 12.8 | 0.01953 | 8.97 | −24.70 | 2.22 | | |
| | 14.0 | 0.01786 | 8.79 | −26.21 | 5.36 | | |
| | 14.6 | 0.01712 | 8.51 | −28.56 | 7.28 | | |

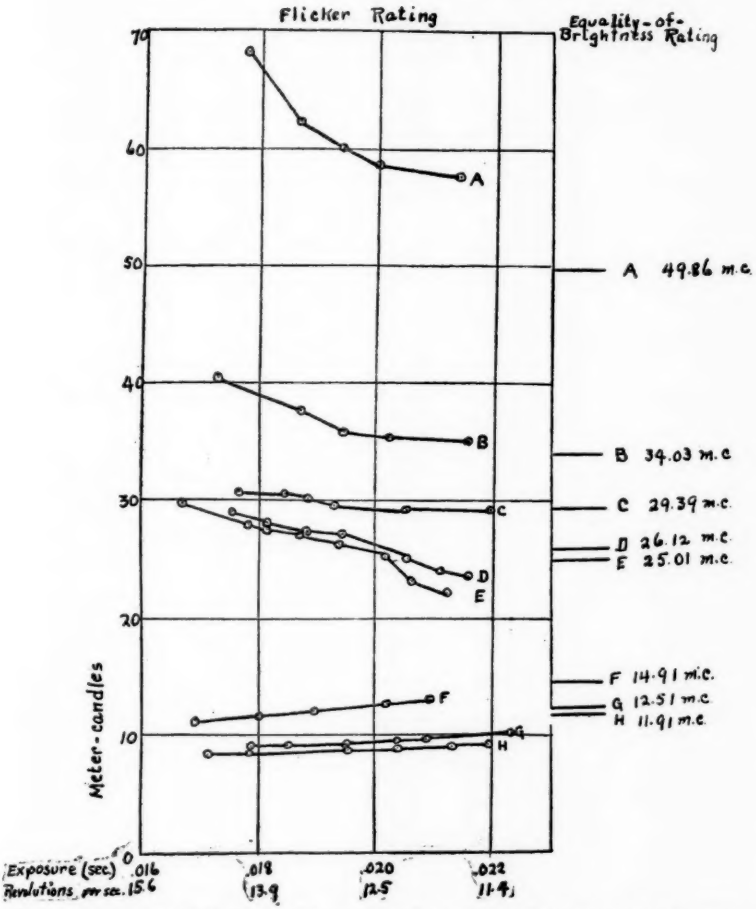


FIG. 1. Showing the effect of varying the speed of rotation of the disc on the disagreement of the flicker and equality-of-brightness ratings at different intensities for red

TABLE II

Showing the Effect of Speed of Rotation of the Disc on the Type and Amount of Disagreement of the Results by the Equality-of-Brightness and Flicker Methods

Color: Y (579$m\mu$)

| Photometric Rating Equality-of-Brightness Method | Rev. per Sec. Flicker Disc | Length of Individual Exposures | Photometric Rating Flicker Method | Difference between the Two Methods | Sensitivity Flicker Method | Change Produced by Variation of Speed | |
|---|---|---|---|---|---|---|---|
| m.c. | | sec. | m.c. | % | % | m.c. | % |
| 49.86 | 11.9 | 0.02101 | 59.62 | +19.57 | 2.27 | 11.80 | 19.92 |
| | 12.6 | 0.01984 | 61.74 | +23.83 | 0.58 | | |
| | 13.2 | 0.01894 | 63.21 | +26.77 | 2.04 | | |
| | 13.8 | 0.01812 | 65.13 | +30.62 | 7.10 | | |
| | 14.4 | 0.01736 | 71.42 | +43.24 | 10.16 | | |
| 29.39 | 11.9 | 0.02101 | 32.59 | +10.88 | 1.66 | 3.97 | 12.19 |
| | 12.2 | 0.02049 | 32.87 | +11.84 | 0.64 | | |
| | 12.7 | 0.01969 | 34.03 | +15.78 | 1.73 | | |
| | 13.2 | 0.01894 | 34.97 | +18.98 | 3.75 | | |
| | 14.0 | 0.01786 | 36.56 | +24.39 | 6.93 | | |
| 26.12 | 11.3 | 0.02212 | 25.88 | − 0.92 | 1.51 | 3.75 | 14.48 |
| | 11.9 | 0.02101 | 26.12 | 0 | 0.57 | | |
| | 12.2 | 0.02049 | 26.53 | + 1.57 | 3.39 | | |
| | 12.7 | 0.01969 | 27.68 | + 5.97 | 3.79 | | |
| | 13.0 | 0.01923 | 28.80 | +10.26 | 5.73 | | |
| | 13.5 | 0.01852 | 29.53 | +13.06 | 8.04 | | |
| 25.01 | 11.9 | 0.02101 | 24.69 | − 1.28 | 1.42 | 5.96 | 31.83 |
| | 12.2 | 0.02049 | 24.02 | − 3.96 | 0.46 | | |
| | 12.9 | 0.01938 | 23.80 | − 4.84 | 2.65 | | |
| | 13.2 | 0.01894 | 21.40 | −14.44 | 6.54 | | |
| | 13.8 | 0.01812 | 19.67 | −21.36 | 9.91 | | |
| | 14.1 | 0.01773 | 18.73 | −25.12 | 13.08 | | |
| 14.91 | 10.9 | 0.02294 | 12.47 | −16.37 | 1.36 | 1.29 | 11.53 |
| | 11.5 | 0.02174 | 12.27 | −17.71 | 0.33 | | |
| | 12.0 | 0.02083 | 12.14 | −18.58 | 1.24 | | |
| | 12.8 | 0.01953 | 11.83 | −20.67 | 1.83 | | |
| | 13.4 | 0.01866 | 11.46 | −23.15 | 5.41 | | |
| | 14.3 | 0.01748 | 11.18 | −25.03 | 10.37 | | |
| 12.51 | 11.2 | 0.02232 | 10.15 | −18.86 | 1.87 | 1.93 | 20.84 |
| | 11.35 | 0.02203 | 9.85 | −21.26 | 0.41 | | |
| | 11.45 | 0.02183 | 9.69 | −22.53 | 0.62 | | |
| | 11.6 | 0.02155 | 9.46 | −24.37 | 1.17 | | |
| | 11.8 | 0.02119 | 9.40 | −24.85 | 8.48 | | |
| | 12.2 | 0.02049 | 9.22 | −26.29 | 16.09 | | |
| 11.91 | 10.9 | 0.02294 | 9.51 | −20.16 | 0.84 | 0.52 | 5.78 |
| | 11.1 | 0.02252 | 9.38 | −21.25 | 0.32 | | |
| | 11.6 | 0.02155 | 9.30 | −21.92 | 3.32 | | |
| | 11.7 | 0.02137 | 9.09 | −23.69 | 4.40 | | |
| | 12.4 | 0.02016 | 8.99 | −24.53 | 11.21 | | |

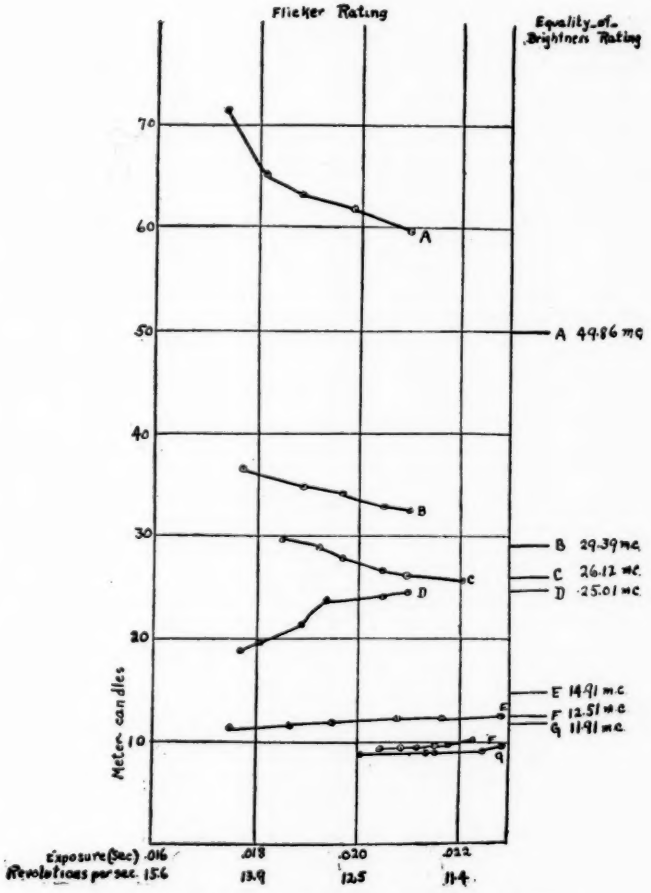


FIG. 2. Effect of speed of rotation of the disc for yellow

TABLE III

Showing the Effect of Speed of Rotation of the Disc on the Type and Amount of Disagreement of the Results by the Equality-of-Brightness and Flicker Methods

Color: G (515$m\mu$)

| Photometric Rating Equality-of-Brightness Method | Rev. per Sec. Flicker Disc | Length of Individual Exposures | Photometric Rating Flicker Method | Difference between the Two Methods | Sensitivity Flicker Method | Change Produced by Variation of Speed | |
|---|---|---|---|---|---|---|---|
| m.c. | | sec. | m.c. | % | % | m.c. | % |
| 49.86 | 11.9 | 0.02101 | 43.36 | −13.04 | 1.45 | 6.17 | 16.60 |
| | 12.3 | 0.02033 | 42.01 | −15.74 | 0.47 | | |
| | 13.4 | 0.01866 | 39.86 | −20.06 | 2.31 | | |
| | 14.3 | 0.01748 | 37.62 | −24.55 | 7.55 | | |
| | 14.8 | 0.01689 | 37.19 | −25.41 | 14.20 | | |
| 29.39 | 12.0 | 0.02083 | 24.69 | −15.99 | 1.82 | 2.91 | 13.36 |
| | 12.4 | 0.02016 | 23.80 | −19.02 | 0.55 | | |
| | 12.7 | 0.01969 | 23.48 | −20.19 | 3.07 | | |
| | 13.2 | 0.01894 | 22.76 | −22.56 | 6.76 | | |
| | 13.5 | 0.01852 | 21.78 | −25.89 | 14.00 | | |
| 26.12 | 11.5 | 0.02174 | 22.20 | −15.01 | 1.76 | 3.41 | 17.63 |
| | 11.9 | 0.02101 | 21.71 | −16.89 | 0.45 | | |
| | 12.6 | 0.01984 | 21.22 | −18.77 | 2.54 | | |
| | 13.3 | 0.01880 | 21.04 | −19.46 | 4.99 | | |
| | 13.7 | 0.01825 | 19.92 | −23.75 | 11.04 | | |
| | 14.4 | 0.01736 | 19.35 | −25.92 | 11.94 | | |
| 25.01 | 10.5 | 0.02381 | 21.40 | −14.44 | 2.52 | 5.71 | 36.37 |
| | 11.8 | 0.02119 | 20.72 | −17.16 | 0.34 | | |
| | 12.35 | 0.02024 | 20.51 | −18.00 | 2.49 | | |
| | 12.75 | 0.01961 | 18.46 | −26.20 | 6.23 | | |
| | 13.5 | 0.01852 | 17.86 | −28.60 | 7.45 | | |
| | 14.1 | 0.01773 | 15.69 | −37.28 | 7.89 | | |
| 14.91 | 11.3 | 0.02212 | 13.70 | − 8.10 | 1.10 | 1.23 | 9.86 |
| | 11.5 | 0.02174 | 13.60 | − 8.79 | 0.37 | | |
| | 11.9 | 0.02101 | 13.51 | − 9.39 | 1.11 | | |
| | 12.5 | 0.020 | 13.33 | −10.60 | 3.30 | | |
| | 13.0 | 0.01923 | 13.06 | −12.41 | 3.90 | | |
| | 13.6 | 0.01838 | 12.72 | −14.69 | 8.17 | | |
| | 14.0 | 0.01786 | 12.47 | −16.37 | 11.95 | | |
| 12.51 | 11.1 | 0.02252 | 11.96 | − 4.40 | 0.68 | 1.12 | 10.33 |
| | 11.3 | 0.02212 | 11.92 | − 4.72 | 0.34 | | |
| | 11.6 | 0.02155 | 11.72 | − 6.31 | 0.69 | | |
| | 12.2 | 0.02049 | 11.39 | − 8.95 | 3.07 | | |
| | 12.6 | 0.01984 | 11.11 | − 9.11 | 9.27 | | |
| | 13.4 | 0.01866 | 10.84 | −13.34 | 13.29 | | |
| 11.91 | 10.8 | 0.02315 | 11.99 | + 0.67 | 1.08 | 1.48 | 14.07 |
| | 11.2 | 0.02232 | 11.91 | 0 | 0.34 | | |
| | 11.4 | 0.02193 | 11.79 | − 1.01 | 0.42 | | |
| | 11.6 | 0.02155 | 11.52 | − 3.28 | 1.74 | | |
| | 11.8 | 0.02119 | 11.43 | − 4.03 | 3.50 | | |
| | 11.9 | 0.02101 | 11.25 | − 5.54 | 5.96 | | |
| | 12.4 | 0.02016 | 10.51 | −11.76 | 12.55 | | |
| 10.20 | 10.6 | 0.02358 | 11.39 | +11.66 | 0.97 | 1.19 | 11.66 |
| | 11.0 | 0.02273 | 11.18 | + 9.60 | 0.36 | | |
| | 11.5 | 0.02174 | 10.77 | + 5.59 | 1.21 | | |
| | 11.8 | 0.02119 | 10.51 | + 3.04 | 2.28 | | |
| | 12.4 | 0.02016 | 10.20 | 0 | 6.76 | | |

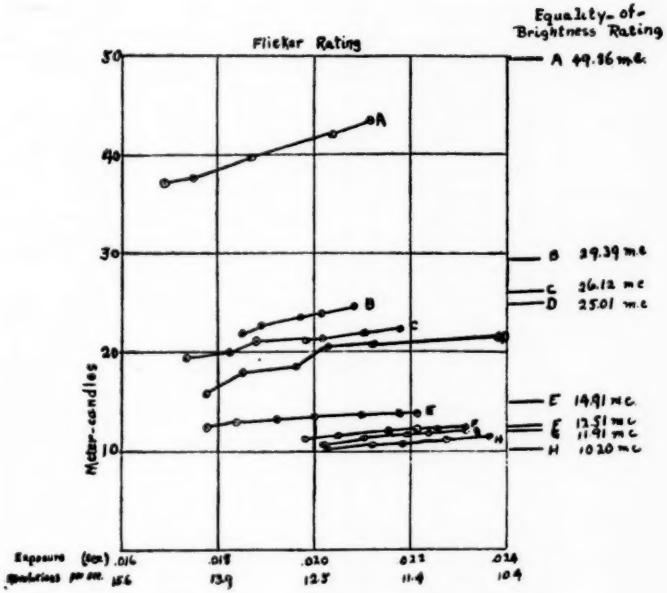


FIG. 3. Effect of speed of rotation of the disc for green

TABLE IV

Showing the Effect of Speed of Rotation of the Disc on the Type and Amount of Disagreement of the Results by the Equality-of-Brightness and Flicker Methods

Color: B (466$m\mu$)

| Photometric Rating Equality-of-Brightness Method | Rev. per Sec. Flicker Disc | Length of Individual Exposures | Photometric Rating Flicker Method | Difference between the Two Methods | Sensitivity Flicker Method | Change Produced by Variation of Speed | |
|---|---|---|---|---|---|---|---|
| m.c. | | sec. | m.c. | % | % | m.c. | % |
| 49.86 | 12.0 | 0.02083 | 42.52 | −14.72 | 2.28 | 5.33 | 14.34 |
| | 12.3 | 0.02033 | 41.55 | −16.67 | 0.58 | | |
| | 12.9 | 0.01938 | 41.07 | −17.63 | 1.80 | | |
| | 13.2 | 0.01894 | 40.33 | −19.11 | 3.45 | | |
| | 13.9 | 0.01799 | 39.40 | −20.98 | 4.52 | | |
| | 14.4 | 0.01736 | 38.49 | −22.80 | 9.65 | | |
| | 16.2 | 0.01543 | 37.19 | −25.41 | 22.57 | | |
| 29.39 | 11.7 | 0.02137 | 22.96 | −21.87 | 1.78 | 3.87 | 20.28 |
| | 12.0 | 0.02083 | 22.55 | −23.27 | 0.53 | | |
| | 12.4 | 0.02016 | 21.87 | −25.58 | 1.28 | | |
| | 12.8 | 0.01953 | 21.48 | −26.91 | 6.10 | | |
| | 13.3 | 0.01880 | 21.22 | −27.79 | 7.30 | | |
| | 13.9 | 0.01799 | 21.13 | −28.10 | 10.64 | | |
| | 14.3 | 0.01748 | 19.09 | −35.04 | 17.82 | | |
| 26.12 | 11.85 | 0.02110 | 20.24 | −22.52 | 1.19 | 3.20 | 18.78 |
| | 12.2 | 0.02049 | 20.00 | −23.43 | 0.35 | | |
| | 12.8 | 0.01953 | 19.75 | −24.40 | 1.21 | | |
| | 13.4 | 0.01866 | 18.88 | −27.73 | 6.89 | | |
| | 13.9 | 0.01799 | 18.14 | −30.56 | 8.87 | | |
| | 14.4 | 0.01736 | 17.04 | −34.78 | 18.49 | | |
| 25.01 | 11.7 | 0.02137 | 19.10 | −23.64 | 0.84 | 3.98 | 26.31 |
| | 12.0 | 0.02083 | 18.94 | −24.28 | 0.32 | | |
| | 12.2 | 0.02049 | 18.70 | −25.24 | 0.80 | | |
| | 12.45 | 0.02008 | 18.58 | −25.72 | 2.37 | | |
| | 13.0 | 0.01923 | 17.58 | −29.72 | 3.81 | | |
| | 13.4 | 0.01866 | 17.17 | −31.36 | 4.72 | | |
| | 13.7 | 0.01825 | 16.53 | −33.92 | 9.80 | | |
| | 14.0 | 0.01786 | 15.12 | −39.56 | 25.58 | | |
| 14.91 | 11.7 | 0.02137 | 15.01 | + 0.67 | 1.13 | 1.35 | 12.69 |
| | 12.0 | 0.02083 | 14.91 | 0 | 0.34 | | |
| | 12.4 | 0.02016 | 14.66 | − 1.67 | 0.69 | | |
| | 12.9 | 0.01938 | 14.66 | − 1.67 | 0.98 | | |
| | 13.6 | 0.01838 | 14.66 | − 1.67 | 4.05 | | |
| | 14.1 | 0.01773 | 14.66 | − 1.67 | 5.25 | | |
| 12.51 | 11.4 | 0.02193 | 13.60 | + 8.71 | 1.03 | 1.20 | 8.82 |
| | 11.6 | 0.02155 | 13.79 | +10.23 | 0.36 | | |
| | 12.2 | 0.02049 | 14.18 | +13.34 | 1.27 | | |
| | 12.8 | 0.01953 | 14.45 | +15.50 | 3.11 | | |
| | 13.4 | 0.01866 | 14.66 | +17.18 | 5.25 | | |
| | 14.2 | 0.01761 | 14.66 | +17.18 | 7.44 | | |
| 11.91 | 11.6 | 0.02155 | 12.89 | + 8.23 | 1.09 | 1.39 | 10.97 |
| | 11.8 | 0.02119 | 13.20 | +10.83 | 0.23 | | |
| | 12.35 | 0.02024 | 13.42 | +12.68 | 2.01 | | |
| | 13.0 | 0.01923 | 13.53 | +13.61 | 5.76 | | |
| | 13.6 | 0.01838 | 13.89 | +16.63 | 11.45 | | |
| | 14.7 | 0.01701 | 14.28 | +19.91 | 17.15 | | |

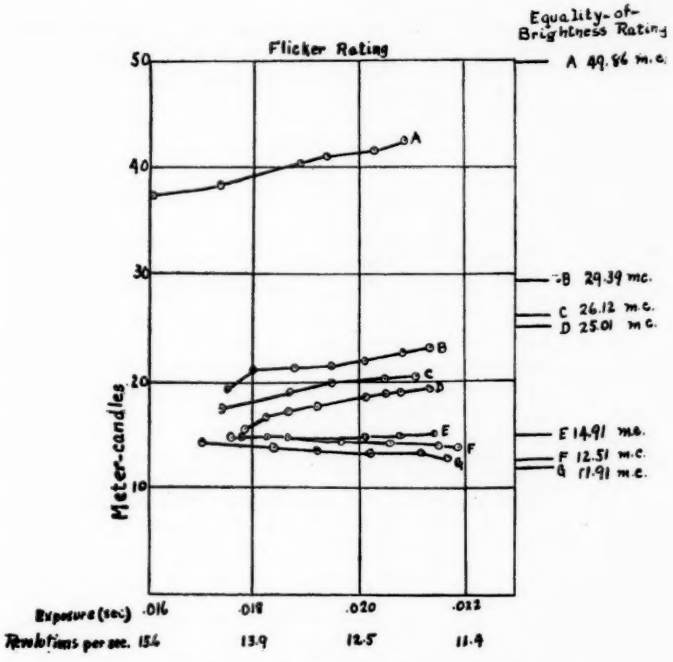


FIG. 4. Effect of speed of rotation of the disc for blue

The results of these Tables for 50, 25 and 12.5 m.c. should be studied in connection with the rise-of-sensation curves[2] for these intensities. However, it is a somewhat difficult task in this case to make even roughly the comparisons needed. That is, while the colors were set to give 50, 25 and 12.5 m.c. by the equality-of-brightness rating, the white light which matches this by the method of flicker at the different speeds would not have been given these ratings. The intensities used, then, for the white light in this Study of the effect of speed are only approximately represented by the curves for the rise of sensation for white light at 50, 25 and 12.5 m.c.

[2]Figg. 1-4, The Effect of Varying the Intensity of Light on the Disagreement of Flicker and Equality-of-Brightness Photometry for Lights of Different Composition, *supra*.

# THE AGREEMENT OF FLICKER AND EQUALITY-OF-BRIGHTNESS PHOTOMETRY WHEN THE SAME LENGTHS OF EXPOSURE ARE USED IN BOTH METHODS

By C. E. Ferree and Gertrude Rand, Bryn Mawr College

In the two previous papers[1] it has been shown that the effect of varying the intensity of light and the speed of rotation of the exposure disc on the disagreement of the results obtained by the flicker and equality-of-brightness methods of photometry is just what would be expected from time of exposure as the cause of the disagreement. As a final test whether the disagreement is due to the difference in the time during which the eye is exposed to the lights compared, it was decided to make the comparison using equal lengths of exposure by both methods. That is, the exposure of the eye in the equality-of-brightness method was cut down to equal that used in the method of flicker. These short exposures were given by means of the rotary tachistoscope described in one of the earlier papers of this series[2], provided however with only one pair of sectored discs. The photometric field was the same as that used in the previous equality-of-brightness comparisons. Some apprehension might be felt as to the sensitivity of the equality-of-brightness comparison with these short exposures. The judgment, however, was not difficult, because the saturation of the colors was greatly reduced by the brevity of the exposure, and the brightness comparison was rendered much less uncertain thereby. The sensitivity of the determination ranged from 1.3 to 2.7%.

The determinations were made for these values of exposure at 50, 25 and 12.5 m.c. In every case the flicker rating fell within the small range of values that were called equal in the short-exposure equality-of-brightness judgment. The results of this work are given in Table I. The general trend of these results has been confirmed with several *O*s.

[1]The Effect of Varying the Intensity of Light on the Disagreement of Flicker and Equality-of-Brightness Photometry for Lights of Different Composition; The Effect of Speed of Rotation of the Disc on the Disagreement of Flicker and Equality-of-Brightness Photometry for Lights of Different Composition and Intensity, *supra*.

[2]Flicker Photometry and the Lag of Visual Sensation, this Journal, 35, 1924, 209-216.

It will be seen from the results given in Table I that agreement was obtained between the two methods within the limits of sensitivity of the judgment. In every case the flicker result fell within the small range of values that was judged equal by the equality-of-brightness method. As was stated in an earlier paper, this might well be considered the most important step in the argument as to the cause of the disagreement between the two methods of photometry. It has been added as a final confirmation, not only to show that difference in time of exposure is the cause of the disagreement between the two methods, but also to ascertain whether there was any detectable effect of summation on the disagreement due to the succession of impressions in the method of flicker. Since agreement of result is obtained by the two methods under this condition, it seems reasonable to conclude that no considerable effect can be attributed to summation.

TABLE I

Showing Agreement of the Results by the Flicker and Equality-of-Brightness Methods when the Same Length of Exposure is used in Both Methods

| Color | Photometric Rating Equality-of-Brightness Method | Photometric Rating Flicker Method | Length of Exposure Flicker Method | Rating by Equality-of-Brightness Method with Exposures Equal to those used in Flicker Method |
|---|---|---|---|---|
| | m.c. | m.c. | sec. | m.c. |
| R | 49.86 | 58.78 | 0.020 | 57.48 - 60.05 |
| (675$m\mu$) | 25.01 | 23.37 | 0.02058 | 23.20 - 23.80 |
| | 12.51 | 9.69 | 0.02083 | 9.61 - 10.02 |
| Y | 49.86 | 61.74 | 0.01984 | 60.05 - 62.79 |
| (579$m\mu$) | 25.01 | 24.02 | 0.02049 | 23.80 - 24.43 |
| | 12.51 | 9.85 | 0.02203 | 9.66 - 10.19 |
| G | 49.86 | 42.01 | 0.02033 | 40.79 - 42.85 |
| (515$m\mu$) | 25.01 | 20.72 | 0.02119 | 20.28 - 21.18 |
| | 12.51 | 11.92 | 0.02212 | 11.68 - 12.20 |
| B | 49.86 | 41.55 | 0.02033 | 40.60 - 42.83 |
| (466$m\mu$) | 25.01 | 18.94 | 0.02083 | 18.65 - 19.44 |
| | 12.51 | 13.79 | 0.02155 | 13.67 - 14.17 |

In concluding this series of articles it may not be out of place to discuss still further two questions which may arise in the mind of the reader.

(1) The determinations for only a small part of a curve for the rise of sensation could be made at a single sitting. How, then, could one put together in a single curve determinations made on different days, and say that the differences found were due to changes in the length of exposure and not to some variations in the condition or sensitivity of the eye? Our reply is

that, in cases of this kind, where a number of results taken at different times have to be compared with reference to the effect of a given variable, the same type of precaution has to be applied to the use of the eye as is exercised when a sensitivity tester is used in connection with a galvanometer or other physical instrument whose sensitivity is liable to vary from time to time. That is, on resuming the work at any particular sitting, it had first to be determined whether the curve made at the preceding sitting could be picked up or duplicated for a few points back. When this could be done, we felt justified in assuming that the eye had not changed its characteristics of response with reference to the function investigated; and the work was allowed to continue on that day. When the curve could not be picked up, determinations were not made on that day. In no other way than this could curves of the regularity of those given in our paper have been obtained. Furthermore, each value that went into the curve as the increment of time required to produce a just noticeable change in sensation was checked at each sitting by a number of repetitions. Done in this way, not more than 5 or 6 points of the curve were determined in a single day. Few pieces of work in psychological optics require more care than this work has required, and few have received or will receive more care than it has received. We can say quite definitely, then, that we consider these curves throughout definitely established for the *O* in question. However, as we have stated earlier in the series of papers, another *O* or group of *O*s picked at random may be found to show a different type of result. But in case a different type of *O* were found, no conclusion could be drawn pertinent to the problem in hand until his curves for the rise of sensation were compared with his photometric results by the two methods. Curves very similar to ours in general characteristics were determined by M. A. Bills[3] in the Bryn Mawr laboratory in 1917 for lights of a lower range of intensities.

(2) The rise-of-sensation curves show the relative levels attained by the two sensations only during the first exposure given by the flicker disc. Can it be assumed that these relations of level persist after the second, third, etc., exposure? In short, are the relative levels to which the sensations rise in the single exposure modified by the succession of exposures occurring in the use of the flicker method? It was just to answer this question that we planned our last experiment,—a comparison of the results of the flicker with the equality-of-brightness method when using for the equality-of-brightness method a single exposure of the length of the individual exposures occurring in the method of

[3]The Lag of Visual Sensation in Relation to Wave-length and Intensity of Light, *Psych. Mon.*, 28, 101 pp.

flicker. This, it seems to us, should give the answer direct to that question. That is, one of the terms compared was the photometric balance obtained by a succession of exposures; the other, that obtained by a single exposure of the length of the individual exposures occurring in flicker, in accordance with the conditions of the question. If agreement were obtained, it seems fair to assume that the relation of levels attained by the two sensations in a single exposure could not have been significantly altered from any cause *whatsoever* in the succession of exposures. This does not mean, of course, that neither sensation has risen to a higher level in the succession of exposures than it attained in the single exposure. It means only that, in rising, the relation of level has not changed by a detectable amount,—in other words, that the sensation which lags the more in a single exposure has not gained on the other sensation in the succession of exposures. Why should it? If an *O* should be found for whom consistently no disagreement occurs in the two ratings,—and we have never heard that an *O* exists for whom this is true over any considerable range of wave lengths at any intensity of light,—there are, of course, two possibilities of explanation: either that there is no differential lag in sensation for the lights of different wave-length for this *O;* or that there is a compensating, differentially modifying or summating effect on the rates of rise in the succession of exposures. Of these two possibilities our results indicate that the former is by far the more probable. That is, at the intensities at which agreement occurred separately for each of the pairs of lights used, the sensations had risen in the single exposure, so far as could be told, to the same level.

In brief, the comparison of the difference in level to which the two sensations are allowed to rise with the type and amount of disagreement of the photometric ratings for the same *O*, state of adaptation of the eye, etc., for the different intensities of light employed, furnishes strong circumstantial evidence in support of our theory that differential lag for wave-length and intensity is the cause of the disagreement in the photometric ratings. The argument is supplemented and completed by the concordant evidence given by the effect of change in the relative lengths of exposure given to the two lights on the type and amount of the disagreement; by the effect of change of speed of rotation of the flicker disc; and particularly by the fact that agreement was found when the same lengths of exposure were used in the two methods.

# THE APPARENT WEIGHT OF COLOR AND CORRELATED PHENOMENA[1]

By MARION MONROE

## *Introduction*

The three experiments described in this Study are concerned with a determination of the factors effective in the phenomenon of "weight" as frequently attributed to color, and with an investigation of the influence of those factors on judgments of aesthetic proportion and on judgments involved in bisecting a line. The problems dealt with may be stated in the form of the following questions. (1) Upon what factor, or factors, brightness, chroma, saturation, relation to background, does the apparent heaviness of a color depend? (2) To what extent do these factors influence judgments of aesthetic proportion? (3) Do these factors also influence one's judgment in bisecting a line?

In mechanical weight, two balls, attached to the ends of a rod, can be balanced on a fulcrum. The fulcrum must necessarily be placed nearer to the heavier ball. The distance between the fulcrum and each ball may serve as an indication of the relative difference between the weights of various pairs of balls. The weight of colors, being a subjective phenomenon, can be determined only by comparisons between pairs of colors as to which color appears to be the heavier. This method does not give an indication of quantitative difference in weight, that is, of how much heavier one member of the pair of colors appears than the other. If, however, two colored circles of equal size were placed at the ends of a line, the judgment as to the place where an imaginary fulcrum would need to be placed on the line in order for the colors to appear balanced in weight would give a means of measuring the difference between the apparent weights of the colors in numerical terms. The values thus assigned to the difference in weight between colors would be comparable for all pairs of colors and for all persons. A judgment could be made also, by using the same line, as to the most pleasing proportion and as to the apparent center of the line.

An apparatus, represented in Fig. 1, was constructed by means of which judgments of this nature could be made conveniently. A brass plate was fitted with narrow brass strips around the sides and lower edge in such a manner as to form a frame for exposing the cards employed as backgrounds for the colors. The cards were held by these strips firmly and securely in place during use, but could be removed easily and replaced when different backgrounds were desired. Three cards were used for backgrounds, *W*, medium *Gr* (grey), and *Bk*, each made from a thin but stiff quality of bristol

[1]This experiment was conducted in the Psychological Laboratory of the University of Chicago. The writer is indebted to Professor Harvey A. Carr for many helpful suggestions during the experimental work and for advice in the interpretation of the data, and to Dr. E. S. Robinson and Dr. F. A. Kingsbury for critical reading of the manuscript.

board cut 9 by 12 in. In the middle of each card a horizontal line, 20 cm. in length, was ruled in black ink in the case of the *W* and *Gr* cards and in white ink in the case of the *Bk* card. With the extremities of the line taken as centers, two circular holes, 1/2 in. in diam., were punched with a steel die constructed for that purpose. These two circles and the line joining them were of a size convenient to be apprehended as a unit from the position of the *S*. The brass plate with the attached framework for exposing the cards was supported in a vertical position by iron uprights screwed to a table, and adjusted in height so that the figure of line and circles was on a level with the eyes of the seated *S* and at a distance from him of approximately arm's length.

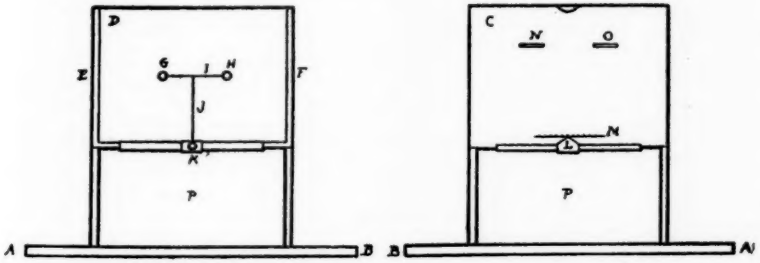


Fig. 1

**Front and Back Views of Exposure Apparatus**

| | | | |
|---|---|---|---|
| AB | Top of table | J | Brass pointer |
| C | Brass plate | K | Handle of pointer |
| D | Background card | L | Brass finger |
| EF | Frame | M | Millimeter scale |
| GH | Circular holes | NO | Slits for exposing colors |
| I | Ruled line | P | Screen |

A small brass pointer, fitting snugly over the card and extending upward until it just touched the ruled line, slid freely back and forth along a brass rod at the base of the frame. A rubber handle was attached to the pointer at its base, and by means of this the *S* moved the pointer to any desired position along the length of the line. Fastened to the lower attachment of the pointer to the rod, a small brass finger extended down under the plate and upward on the rear side to a mm. scale, where it indicated the exact position of the pointer in relation to the center of the line, and thus enabled the *E* to record data out of sight and knowledge of the *S*.

On the back of the plate two slits were milled through the brass at an angle sloping downward but placed slightly above the positions corresponding to the circular holes in the card when fitted into the frame. Strips of colored paper were inserted through these slits and made to lie flat between the card and the brass plate. From the front the papers were exposed through the openings in the card, thus giving the appearance of colored circles at the ends of the line. In this manner, the background card, pointer, line, illumination, distance from *S*, and any possible errors due to any of these factors, were kept constant throughout each series of pairs of colors.

The experiments were conducted in a dark room with artificial light in order to avoid the variability of daylight. A drop-light was suspended above the chair of the *S* at such an angle as to throw the light directly and without shadow upon the colors.

The standard colored papers of the Abbott Educational Company were employed. Three series of paired colors were presented, consisting of *R*, *G*, *B*, and *Y*, varying in brightness, chroma, and saturation. The first series was composed of pairs of colors highly similar in chroma and saturation but differing greatly in brightness. The second series was composed of pairs of colors having approximately the same degree of saturation but varying in chroma. An attempt was made to choose colors in this series nearly equal in brightness; but there was, nevertheless, a varying amount of brightness inherent in each chroma of good saturation which it was impossible to equalize and still to maintain an approximation of equal saturation between pairs of colors. The third series consisted of pairs of colors highly similar in chroma and brightness but differing greatly in saturation. The choice of colors was made by the writer who has had experience with color in the Psychological Laboratory as well as several years' training in the use of color in art schools. While it was impossible to control exactly the factors of brightness, chroma, and saturation, with the pigment colors provided in the assortment, the variable factor in each series was always very great and any difference in the other factors was slight and not very apparent.

The pairs of colors used in each series were as follows. Series I, Brightness difference: Light *R* - Dark *R*, Light *G* - Dark *G*, Light *B* - Dark *B*, Light *Y* - Dark *Y*. Series II, Chroma difference: *R* - *Y*, *R* - *B*, *R* - *G*, *G* - *B*, *G* - *Y*, *Y* - *B*. Series III, Saturation difference: Saturated *R* - Less saturated *R*, Saturated *G* - Less saturated *G*, Saturated *B* - Less saturated *B*, Saturated *Y* - Less saturated *Y*.

All of the pairs of colors from these three series were mixed together at random and a chance order of presentation was drawn so that the pairs of colors did not follow one another in the order of the series just described or have any regular right-left relationship between the members of each pair. After the sequence and right-left positions had been determined by chance, they were kept constant for all *S*s. After judgments for each sequence had been completed, the pairs of colors were presented again 24 hrs. later with a reversed right-left relationship. In this manner any errors due to preferences in terms of position were corrected.

Two groups of *S*s took part in the experiments. The first group consisted of seven graduate students in the department of Psychology in the University of Chicago. The second group consisted of three graduate and four undergraduate students in the same department. There were four women and three men in each group. All of the *S*s were naïve as to the purpose of the experiment. Only two judgments were made by each *S* for each pair of colors, one with the normal and one with the reversed right-left relationship between each pair of colors. For this reason, group results are considered rather than records of individuals. Each numerical value is the average of 14 judgments and is valid only as an indication of the general trend of judgments and as a suggestion of results that might be expected if the values were refined by larger groups of *S*s and a larger number of individual judgments.

The sittings took place on successive days and followed the conditions of the experiments in the following order.

Group I

Sitting 1. Experiment I, *W* background, normal position
Experiment II, *W* background, normal position
Experiment III, *W* background, normal position

Sitting 2. Experiment I, *W* background, reversed position
Experiment II, *W* background, reversed position
Experiment III, *W* background, reversed position

Sitting 3. Experiment I, *Gr* background, normal position
Experiment II, *Gr* background, normal position
Experiment III, *Gr* background, normal position

Sitting 4. Experiment I, *Gr* background, reversed position
Experiment II, *Gr* background, reversed position
Experiment III, *Gr* background, reversed position

Sitting 5. Experiment I, *Bk* background, normal position
Experiment II, *Bk* background, normal position
Experiment III, *Bk* background, normal position

Sitting 6. Experiment I, *Bk* background, reversed position
Experiment II, *Bk* background, reversed position
Experiment III, *Bk* background, reversed position

Group II

Sitting 1. Experiment III, *W* background, normal position
Experiment II, *W* background, normal position

Sitting 2. Experiment III, *W* background, reversed position
Experiment II, *W* background, reversed position

### *Experiment I*

The purpose of this experiment was to discover to what extent the apparent heaviness of a color is a function of its brightness, chroma, saturation, or relation to background.

The following typewritten directions were handed to the *S* at the beginning of the experiment. "Imagine the black line to be a rigid iron bar connecting two heavy colored balls and supported by the pointer as a fulcrum. Move the pointer along the line until the two balls appear to be exactly balanced in weight." In the case of the black background the word "white" was substituted for "black" in reference to the line.

The results of this experiment are given in Tables I, II and III. The numerical values refer to the average difference in mm. between the left and right judgments of the *S*s of Group I for each pair of colors in the various series. The initial letters of the colors are used as symbols to denote the pair of colors for which the judgment is made and to denote the direction of the difference. For example, in Table I, "*DR - LR*" refers to the pair of colors Dark Red - Light Red, and "5.2 to *DR*" means that the average position of the pointer was 5.2 mm. nearer to the Dark Red color when the pair of colors appeared balanced in weight, or that Dark Red was judged to be heavier than Light Red. The greater the amount of difference, the more marked is the apparent disparity in weight between the two colors. The average amount of difference between the right and left judgments is given for each pair of colors on each background.

In the series of pairs of colors differing in brightness, as shown in Table I, the darker colors were judged to be heavier than the lighter colors. The results are consistent throughout the series except in the case of Light *Y* on the *Bk* background. In this one instance the light color appeared to be heavier than the dark color, perhaps owing to the extreme contrast between the color and the background. The background affected the degree of heaviness to some extent, particularly in the case of Dark *R*, which grew progressively heavier from *W* to *Gr* to *Bk* background, and in the case of Dark *Y*, which grew progressively lighter, weighing even less than Light *Y* on the *Bk* background.

In the series of pairs of colors differing in chroma, as shown in Table II, the following order of weight was found, beginning with the heaviest color: *B*, *R*, *G*, *Y*. The results are consistent for this order of weight throughout the series. A close similarity is found between the numerical values of the succeeding steps for colors on the *W* and *Bk* backgrounds. The results on the *Gr*

TABLE I*

The Effect of Brightness on the Apparent Weight of Color

| Pair of colors | *W* | *Gr* | *Bk* |
|---|---|---|---|
| Series I | background | background | background |
| DR - LR | 5.2 to DR | 9.0 to DR | 11.2 to DR |
| DG - LG | 5.6 to DG | 6.4 to DG | 3.8 to DG |
| DB - LB | 11.4 to DB | 11.8 to DB | 11.4 to DB |
| DY - LY | 10.0 to DY | 3.6 to DY | 2.0 to LY |

TABLE II

The Effect of Chroma (or Brightness) on the Apparent Weight of Color

| Pair of colors | *W* | *Gr* | *Bk* |
|---|---|---|---|
| Series II | background | background | background |
| B - Y | 9.8 to B | 12.2 to B | 8.2 to B |
| R - Y | 7.2 to R | 6.8 to R | 6.8 to R |
| B - G | 5.6 to B | 11.2 to B | 6.8 to B |
| B - R | 4.4 to B | 2.6 to B | 4.4 to B |
| R - G | 2.8 to R | 5.4 to R | 4.0 to R |
| G - Y | 2.8 to G | 2.2 to G | 2.2 to G |

TABLE III

The Effect of Saturation on the Apparent Weight of Color

| Pair of colors | *W* | *Gr* | *Bk* |
|---|---|---|---|
| Series III | background | background | background |
| Sat R - LSR | 5.8 to LSR | 5.7 to LSR | 0.2 to LSR |
| Sat G - LSG | 3.6 to LSG | 1.9 to LSG | 3.0 to LSG |
| Sat Y - LSY | 6.6 to LSY | 2.9 to LSY | 3.4 to LSY |
| Sat B - LSB | 4.2 to LSB | 3.8 to LSB | 2.2 to LSB |

*The values given in this and in the following tables are the average differences in mm. between the right and left judgments of 7 *Ss* making two judgments, one for each position, for each pair of colors.

background do not fit so closely into the specific scheme suggested on the *W* and *Bk* backgrounds, but are, nevertheless, in harmony with the general order of weight. It would be expected from this order that the greatest disparity in apparent weight should occur between *B* and *Y*; and this expectation is confirmed by the greatest numerical difference between right and left judgments for this pair of colors. The next largest difference should appear between *R* and *Y*, and *B* and *G*, and the smallest difference should be found between *B* and *R*, *R* and *G*, and *G* and *Y*. The data given in Table II consistently conform to this relationship between the apparent weights of these various pairs of colors. Since the order of weight in the series of chroma difference is the same as the order of the inherent brightness of the

chromas themselves, *B* and *R* being darker than *G* and *Y*, it is probable that the results of the second series may be subsumed under the results of the first. The results of both series may be summed up in the general statement: *dark colors appear heavier than light colors.*

In the series of pairs of colors differing in saturation, the less saturated color was judged to appear the heavier. The results, as shown in Table III, are consistent throughout the series and for all backgrounds. While the average of the judgments of the group was invariably toward the less saturated color as heavier, individual judgments in many cases favored the saturated color as appearing to have the greater weight. An example of the consistency between individuals in their judgments of the apparent weight of colors differing in brightness or chroma, as contrasted with the variability of judgments of the apparent weight of colors differing in saturation, is given in Table IV. The individual records here tabulated for a pair of colors in each series are typical of the records obtained throughout each series. In Series I, the *S*s were unanimous in judging the darker colors to appear heavier than the corresponding lighter colors. In Series

TABLE IV

Typical Records Showing the Variation between Individuals in Judgments of the Apparent Weight of Color

| Pair of colors | *S* | Difference between right and left judgment |
|---|---|---|
| Series I | Brown | 11.0 to DR |
| DR - LR | Haffner | 2.0 to DR |
| *Gr* background | Stratton | 4.0 to DR |
| | Bills | 20.0 to DR |
| | Prophet | 14.0 to DR |
| | Miller | 10.0 to DR |
| | Darrow | 2.5 to DR |
| | Average | 9.0 to DR |
| Series II | Brown | 3.0 to R |
| R - Y | Haffner | 7.0 to R |
| *Gr* background | Stratton | 0.0 |
| | Bills | 6.0 to R |
| | Prophet | 9.0 to R |
| | Miller | 12.0 to R |
| | Darrow | 11.0 to R |
| | Average | 6.8 to R |
| Series III | Brown | 16.0 to LSR |
| Sat R - LSR | Haffner | 3.0 to Sat R |
| *Gr* background | Stratton | 3.0 to Sat R |
| | Bills | 6.0 to Sat R |
| | Prophet | 8.0 to LSR |
| | Miller | 23.0 to LSR |
| | Darrow | 5.0 to LSR |
| | Average | 5.7 to LSR |

II, the records here tabulated indicate that *R* consistently appeared heavier than *Y* except in the case of one *S* whose left and right judgments were identical and cancelled each other. In Series III, however, to three of the seven *S*s the saturated color appeared to be heavier than the less saturated color. With the majority of *S*s, the greater saturation of a color seems to make it appear lighter than the less saturated color. The saturated color appears virile and full of an inner energy that seems to lift it of its own force and make it buoyant in comparison with the "lifeless" and "flat" less saturated color. One *S* particularly had trouble with the saturated *G*. "It goes up like a balloon," she said. "It will not stay down and be weighed."

The relation of the color to the background did not affect the apparent weight of color in any regular manner in either the series of chroma or in that of saturation differences. An increase in brightness of the background decreased the apparent heaviness of Dark *R* and increased the apparent heaviness of Dark *Y* in the series of brightness difference. Since the only variable used in the background was brightness, *W*, *Gr* and *Bk*, the greatest opposition between the brightness of color and background occurred in this series with, consequently, the greatest effect upon the apparent weight of the colors.

As an indication of the comparative effectiveness of the three factors, brightness, chroma (or brightness), and saturation, in determining the apparent weight of color, the average values of each series may be cited.

Brightness: 7.6 to the darker color
Chroma (or Brightness): 6.0 to *B*, *R*, *G*
Saturation: 3.6 to the less saturated color

*Experiment II*

The purpose of this experiment was to determine whether the factors that influence the apparent weight of color also influence judgments of aesthetic proportion.

The following typewritten direction was handed to the *S* at the beginning of the experiment. "Move the pointer along the line to the most pleasing position between the two colored circles."

The results of the experiment are given in Tables V, VI, VII, and VIII. As in the previous experiment, the numerical values are the average differences in mm. between the right and left judgments of the *S*s for each pair of colors, and the initial letters of the colors are used as symbols to denote the pair of colors and the direction of the preference.

In the series of pairs of colors varying in brightness, the preferred position was consistently nearer to the heavier color. A comparison of the judgments of aesthetic proportion with the judgments of apparent weight in this series is given in Table V. The averages of the values assigned to aesthetic proportion are greater than those assigned to apparent weight with one exception in the case of one pair of colors.

In the series of pairs of colors differing in chroma, there was very little correspondence between the apparent weight of color and aesthetic proportion. There was a tendency to prefer the pointer near to *G* or *Y*, particularly on the *Gr* background. One *S* remarked: "It does not look right to see *Y* at the extreme end of the line"; and she put the pointer next to it in order to bring it nearer to the center of interest. Another *S* had a decided dislike for the combination of *R* and *G*. "The only way I can like

TABLE V

The Correlation of Judgments of the Apparent Weight of Color and Judgments of Aesthetic Proportion as Dependent upon Brightness of Color

| Pair of colors Series I | Background | Weight | Aesthetic Proportion |
|---|---|---|---|
| DR - LR | *W* | 5.2 to DR | 12.8 to DR |
| DG - LG | *W* | 5.6 to DG | 9.0 to DG |
| DB - LB | *W* | 11.4 to DB | 16.4 to DB |
| DY - LY | *W* | 10.0 to DY | 11.8 to DY |
| DR - LR | *Gr* | 9.0 to DR | 12.8 to DR |
| DG - LG | *Gr* | 6.4 to DG | 12.2 to DG |
| DB - LB | *Gr* | 11.8 to DB | 12.6 to DB |
| DY - LY | *Gr* | 3.6 to DY | 6.6 to DY |
| DR - LR | *Bk* | 11.2 to DR | 10.0 to DR |
| DG - LG | *Bk* | 3.8 to DG | 8.0 to DG |
| DB - LB | *Bk* | 11.4 to DB | 14.8 to DB |
| DY - LY | *Bk* | 2.0 to LY | 7.6 to LY |

TABLE VI

The Correlation of Judgments of the Apparent Weight of Color and Judgments of Aesthetic Proportion as Dependent upon Chroma (or Brightness) of Color

| Pair of colors Series II | Background | Weight | Aesthetic Proportion |
|---|---|---|---|
| B - Y | *W* | 9.8 to B | 7.6 to B |
| R - Y | *W* | 7.2 to R | 8.4 to R |
| B - G | *W* | 5.6 to B | 2.2 to G |
| B - R | *W* | 4.4 to B | 5.0 to B |
| R - G | *W* | 2.8 to R | 2.6 to G |
| G - Y | *W* | 2.8 to G | 14.8 to Y |
| B - Y | *Gr* | 12.2 to B | 4.6 to Y |
| R - Y | *Gr* | 6.8 to R | 2.4 to Y |
| B - G | *Gr* | 11.2 to B | 2.8 to G |
| B - R | *Gr* | 2.6 to B | 5.2 to B |
| R - G | *Gr* | 5.4 to R | 2.6 to G |
| G - Y | *Gr* | 2.2 to G | 4.6 to Y |
| B - Y | *Bk* | 8.2 to B | 8.2 to B |
| R - Y | *Bk* | 6.8 to R | 9.2 to R |
| B - G | *Bk* | 6.8 to B | 6.6 to B |
| B - R | *Bk* | 4.4 to B | 2.0 to B |
| R - G | *Bk* | 4.0 to R | 2.2 to G |
| G - Y | *Bk* | 2.2 to G | 2.6 to G |

them together is to separate them as far as possible." This *S* always preferred the pointer in the center of the line between this pair of colors. The *S*s were influenced in their judgments of aesthetic proportion by the factor of chroma rather than by the factor of the brightness of the chroma, which possibly had operated in the previous judgments of apparent weight. There was not a complete opposition of the influence of chroma upon judgments of aesthetic proportion to the influence of brightness upon the judgments of apparent weight. In about half of the cases, and particularly on the *Bk* background, the direction of the preferred proportion was toward the heavier color. The results of this series are given in Table VI.

In the series of pairs of colors differing in saturation, the position of the pointer was preferred nearer to the heavier color with the one exception of Saturated *B* on the *W* background. A comparison of the values assigned to aesthetic proportion with those assigned to apparent weight is given in Table VII. The

TABLE VII

The Correlation of Judgments of the Apparent Weight of Color and Judgments of Aesthetic Proportion as Dependent upon Saturation of Color

| Pair of colors Series III | Background | Weight | Aesthetic Proportion |
|---|---|---|---|
| Sat R - LSR | *W* | 5.8 to LSR | 0.8 to LSR |
| Sat G - LSG | *W* | 3.6 to LSG | 9.2 to LSG |
| Sat Y - LSY | *W* | 6.6 to LSY | 0.6 to LSY |
| Sat B - LSB | *W* | 4.2 to LSB | 4.0 to Sat B |
| Sat R - LSR | *Gr* | 5.7to LSR | 7.0 to LSR |
| Sat G - LSG | *Gr* | 1.9 to LSG | 3.4 to LSG |
| Sat Y - LSY | *Gr* | 2.9 to LSY | 4.2 to LSY |
| Sat B - LSB | *Gr* | 3.8 to LSB | 11.2 to LSB |
| Sat R - LSR | *Bk* | 0.2 to LSR | 5.0 to LSR |
| Sat G - LSG | *Bk* | 3.0 to LSG | 6.0 to LSG |
| Sat Y - LSY | *Bk* | 3.4 to LSY | 10.4 to LSY |
| Sat B - LSB | *Bk* | 2.2 to LSB | 7.2 to LSB |

values for aesthetic proportion are larger than the values for apparent weight on the *Gr* and *Bk* backgrounds but not on the *W* background. As in the series of brightness difference, there is a close correspondence between the direction of the apparent weight of color and the direction of the preferred proportion.

There is clearly a correlation between the judgments of apparent weight and those of aesthetic proportion in the series of brightness and saturation differences. There is a lack of correlation between the judgments of apparent weight and aesthetic proportion in the series of chroma difference. So far as there is a correlation, the common factors of brightness and saturation operate in both judgments in the same manner. So far as there

is a lack of correlation, the apparent weight of color is probably affected by one factor, the inherent brightness of chroma, while aesthetic proportion is affected by another factor, the chroma, regardless of brightness.

The fact of the correlation might be explained as a result of the direct influence of the apparent weight of color upon the preferred proportion. Since, however, the preferred proportion is not identical with the position of balanced weight but only in the same direction, *i. e.*, toward the heavier color, it is difficult to understand how a direct relationship would operate. That the correlation is due to the similar influence of some common factor in both judgments is a simpler explanation.

TABLE VIII

The Effect of Practice in Judging the Apparent Weight of Color upon Judgments of Aesthetic Proportion

| Pair of colors Series I, II, III | Background | Group I (Practised) | Group II (Unpractised) |
|---|---|---|---|
| DR - LR | *W* | 12.8 to DR | 8.8 to DR |
| DG - LG | *W* | 9.0 to DG | 0.8 to LG |
| DB - LB | *W* | 16.4 to DB | 0.6 to LB |
| DY - LY | *W* | 11.8 to DY | 1.2 to DY |
| B - Y | *W* | 7.6 to B | 5.2 to Y |
| R - Y | *W* | 8.4 to R | 7.4 to Y |
| B - G | *W* | 2.2 to G | 1.2 to G |
| B - R | *W* | 5.0 to B | 12.4 to B |
| R - G | *W* | 2.6 to G | 6.4 to G |
| G - Y | *W* | 14.8 to Y | 10.4 to Y |
| Sat R - LSR | *W* | 0.8 to LSR | 14.0 to LSR |
| Sat G - LSG | *W* | 9.2 to LSG | 20.4 to LSG |
| Sat Y - LSY | *W* | 0.6 to LSY | 0.2 to LSY |
| Sat B - LSB | *W* | 4.0 to Sat B | 11.8 to LSB |

There is a possibility that the close correlation between the apparent weight of color and aesthetic proportion might be due to the fact that the *S*s making the judgments of aesthetic proportion had just previously made judgments of the apparent weight of color and thus had had their attention directed to that phenomenon. It is possible that an attitude toward the colors could result from this experience which might cause the correspondence between the judgments of apparent weight and those of aesthetic proportion. In order to discover the effect of this previous experience and to avoid its influence, the experiment was repeated on a second group of *S*s with only the *W* background. This second group of *S*s had not balanced the colors in weight as the original group had done, but had previously made judgments in bisecting the line between the pairs of colors. The results of the two groups are given in Table VIII for comparison. The group of *S*s who had not made judgments of the

apparent weight of color had less correspondence between that factor and aesthetic proportion than did the original group. There was a greater variation in individual judgments in the second group than in the first, and a much wider range of numerical values. The group of *S*s employed in obtaining the averages given in the Table was not large enough to mitigate the effects of a few instances where the extreme judgment of a single *S* biassed the average of the group in a manner unwarranted by a consideration of the individual records. The judgments toward Light *G* and Light *B* in Series I are examples of this apparent departure from the general trend of judgments made by the original group. If we consider the records of individuals rather than group averages, the majority of the individual judgments made by the second group showed, nevertheless, the same direction of preference as the apparent weight of color. Practice in judging the relative weights of colors increases the correspondence between the apparent weight of color and aesthetic proportion and slightly decreases the individual variability of aesthetic judgments.

### *Experiment III*

The purpose of this experiment was to discover whether the same factors that affect the apparent weight of color also influence the judgments involved in bisecting a line.

The following typewritten direction was handed to the *S* at the beginning of the experiment. "Move the pointer along the line until it is half way between the two colored circles." The *S* was allowed to move the pointer in only one direction, from right to left. Before presenting the next pair of colors after a judgment had been made, and also in cases where the *S* desired to make a correction of a judgment, the *E* moved the pointer back to the right of the line, placing it at varying positions in order to avoid the *S*'s making a judgment on the basis of the extent of movement.

The results of the experiment are given in Tables IX, X, XI, and XII, in which the numerical values, as in the preceding experiments, refer to the average difference in mm. between right and left judgments for each pair of colors, and the color symbols indicate the direction of the difference.

The judgments of the midpoints of the line were biassed toward the heavier color. This illusion was greatest during the early part of the experiment, that is, while the *S*s were using the *W* background. With practice the illusion tended to disappear, although the *S*s were ignorant of the amount and direction of their errors. On the *W* background, the results, as shown in Table IX, are consistently and invariably biassed toward the heavier color. With the *Gr* and *Bk* backgrounds, as shown in Tables X and XI, the heavier color predominates in the majority of cases. This is probably due to the fact that with practice the *S*s gradually disregarded the colors at the end of the line and became more intent upon the line itself. To verify this practice-effect, the experiment was repeated on a second group of *S*s with

TABLE IX

The Correlation of Judgments of the Apparent Weight of Color and Judgments Involved in Bisecting a Line

| Pair of colors Series I, II, III | Background | Weight | Judgment of center of line |
|---|---|---|---|
| DR - LR | *W* | 5.2 to DR | 0.6 to DR |
| DG - LG | *W* | 5.6 to DG | 0.2 to DG |
| DB - LB | *W* | 11.4 to DB | 0.5 to DB |
| DY - LY | *W* | 10.0 to DY | 0.6 to DY |
| B - Y | *W* | 9.8 to B | 0.2 to B |
| R - Y | *W* | 7.2 to R | 0.9 to R |
| B - G | *W* | 5.6 to B | 0.3 to B |
| B - R | *W* | 4.4 to B | 0.1 to B |
| R - G | *W* | 2.8 to R | 0.3 to R |
| G - Y | *W* | 2.8 to G | 0.3 to G |
| Sat R - LSR | *W* | 5.8 to LSR | 1.3 to LSR |
| Sat G - LSG | *W* | 3.6 to LSG | 0.1 to LSG |
| Sat Y - LSY | *W* | 6.6 to LSY | 0.5 to LSY |
| Sat B - LSB | *W* | 4.2 to LSB | 0.3 to LSB |

TABLE X

The Correlation of Judgments of the Apparent Weight of Color and Judgments involved in bisecting a Line after Practice on the White Background

| Pair of colors Series I, II, III | Background | Weight | Judgment of center of line |
|---|---|---|---|
| DR - LR | *Gr* | 9.0 to DR | 0.1 to LR |
| DG - LG | *Gr* | 6.4 to DG | 0.2 to DG |
| DB - LB | *Gr* | 11.8 to DB | 0.3 to DB |
| DY - LY | *Gr* | 3.6 to DY | 0.1 to LY |
| B - Y | *Gr* | 12.2 to B | 0.6 to B |
| R - Y | *Gr* | 6.8 to R | 0.1 to R |
| B - G | *Gr* | 11.2 to B | 0.1 to B |
| B - R | *Gr* | 2.6 to B | 0.4 to R |
| R - G | *Gr* | 5.4 to R | 0.2 to G |
| G - Y | *Gr* | 2.2 to G | 0.3 to Y |
| Sat R - LSR | *Gr* | 5.7 to LSR | 0.1 to LSR |
| Sat G - LSG | *Gr* | 1.9 to LSG | 0.5 to Sat G |
| Sat Y - LSY | *Gr* | 2.9 to LSY | 0.2 to LSY |
| Sat B - LSB | *Gr* | 3.8 to LSB | 0.3 to LSB |

the *W* background. The results of the two groups are given in Table XII for comparison. With the unpractised group of *Ss*, the results of the early part of the experiment were duplicated and the judgments of the midpoints of the line were again found to be consistently biassed toward the heavier color.

There is a close correlation of direction between the judgments of the apparent weight of color and the judgments of the midpoint of the line. This correlation gradually decreases with an increase in practice on the part of the *Ss* in making judgments of the apparent center of the line. The values of the judgments

of the midpoint of the line are much smaller than the values for apparent weight. The *S*s were very much influenced by the line itself in making the judgment and were able to gauge the middle with considerable accuracy. They were only slightly influenced by the factors of brightness, chroma (or brightness), and saturation. The correspondence between the direction of the judgments of the apparent weight of color and the judgments of the midpoint of the line is so consistent that it is evident that the same factors operating in the judgments of apparent weight of color operate in the same manner but to a less degree in the judgments involved in bisecting a line.

TABLE XI

The Correlation of Judgments of the Apparent Weight of Color and Judgments Involved in Bisecting a Line after Practice on the White and Gray Backgrounds

| Pair of colors Series I, II, III | Background | Weight | Judgment of center of line |
|---|---|---|---|
| DR - LR | *Bk* | 11.2 to DR | 0.7 to DR |
| DG - LG | *Bk* | 3.8 to DG | 0.4 to DG |
| DB - LB | *Bk* | 11.4 to DB | 0.1 to LB |
| DY - LY | *Bk* | 2.0 to LY | 0.9 to DY |
| B - Y | *Bk* | 8.2 to B | 0.2 to B |
| R - Y | *Bk* | 6.8 to R | 0.4 to R |
| B - G | *Bk* | 6.8 to B | 0.4 to B |
| B - R | *Bk* | 4.4 to B | 0.6 to B |
| R - G | *Bk* | 4.0 to R | 0.1 to G |
| G - Y | *Bk* | 2.2 to G | 0.2 to G |
| Sat R - LSR | *Bk* | 0.2 to LSR | 0.1 to LSR |
| Sat G - LSG | *Bk* | 3.0 to LSG | 0.1 to Sat G |
| Sat Y - LSY | *Bk* | 3.4 to LSY | 0.8 to LSY |
| Sat B - LSB | *Bk* | 2.2 to LSB | 0.2 to LSB |

TABLE XII

Comparison of the Judgments of Two Different Groups of Subjects in Bisecting a Line

| Pair of colors Series I, II, III | Background | Group I | Group II |
|---|---|---|---|
| DR - LR | *W* | 0.6 to DR | 0.9 to DR |
| DG - LG | *W* | 0.2 to DG | 0.2 to DG |
| DB - LB | *W* | 0.5 to DB | 0.4 to DB |
| DY - LY | *W* | 0.6 to DY | 0.3 to DY |
| B - Y | *W* | 0.2 to B | 0.8 to B |
| R - Y | *W* | 0.9 to R | 1.0 to R |
| B - G | *W* | 0.3 to B | 0.7 to B |
| B - R | *W* | 0.1 to B | 1.0 to B |
| R - G | *W* | 0.3 to R | 0.3 to R |
| G - Y | *W* | 0.3 to G | 0.1 to G |
| Sat R - LSR | *W* | 1.3 to LSR | 1.3 to LSR |
| Sat G - LSG | *W* | 0.1 to LSG | 0.3 to LSG |
| Sat Y - LSY | *W* | 0.5 to LSY | 0.5 to LSY |
| Sat B - LSB | *W* | 0.6 to LSB | 0.0+ to LSB |

*Review of Previous Work*

Comparatively little experimentation has been done in the field of the aesthetics of color, aside from the analysis of the color preferences of groups of *Ss* of different age, sex, and social environment.

The phenomenon of the apparent weight of color was first investigated by Bullough[2], who attempted to test out the "rule" of artists that "the dark colors should stand below the light colors." The reason for this preference he designates as the weight principle, which he thinks is due to an apparent "moreness" of substance in the dark color. More pink added to pink, for example, makes the color pinker, and therefore apparently heavier, because of the increase of pigment. The addition of pigment decreases the luminosity of the color as it increases its apparent weight. The experiment performed consisted of obtaining preferences of *Ss* between pairs of triangles, squares, and circles, the upper half of which was filled with one color and the lower half with another color, or a different shade of the first color. The *S* was presented with two forms, alike except for the reversed position of the colors, and was asked to choose the more pleasing of the two and to give a reason for his preference. Many of the *Ss* were able to trace their preferences immediately to the "weight principle," particularly in cases where different shades of the same color were employed. More variation was found where colors of different "tones," *i. e.*, differing in chroma, were used. Often the subject was at a loss to account for his preference, in which case a number of possible reasons were suggested to him and he chose the one that seemed most appropriate. The majority of the preferences were made for the form having the darker color at the bottom because of its apparently greater weight, and consequent stability.

The influence of color upon the apparent weight of objects was investigated by De Camp[3], who covered a number of wooden and cardboard cubes of exactly the same mechanical weight in each group with colored papers and obtained judgments between various blocks as to which appeared to be the heavier when lifted. In the first experiment, with wooden blocks, he obtained the following arrangement of the blocks according to apparent weight, beginning with the heaviest: *R, W, O, V, G, P, B, Y, Bk.* The second experiment was performed with very light cubes made from bristol board, in order to obtain the maximum effect of the influence of the colors; since the lighter the object, the smaller would be the appreciable difference n weight, and hence the greater any influence of the apparent weight of the colors. According to the second experiment, the order of the apparent weight of the blocks, beginning with the heaviest was: *Bk, Gr, W,* for one series, and *B, Y, R, W,* for another series. There was no agreement with the rule, "light colors - light weight, dark colors, - heavy weight," although the results of the second experiment are more suggestive of that arrangement than are the results of the first experiment. De Camp concludes from the data of this experiment that "seemingly there is no simple correlation between the affective quality of a color and its influence upon apparent weight." The *Ss* were asked also to judge which cubes "looked" heaviest, without actually lifting them, and under these conditions the order of the apparent weight of the cubes beginning with the heaviest, was: *Bk, B, R, Gr, Y, W,* which is in harmony with the results obtained in the present investigation.

The work upon illusions in making judgments in bisecting a line is extensive and generally out of the province of the present investigation. As far as the writer has been able to discover, there has been no study made, previous to this inquiry, of the effect of color upon judgments of this nature.

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[2]E. Bullough, On the Apparent Heaviness of Colors, *Brit. Jour. Psych.*, 2, 1907, 111-152.

[3]J. E. De Camp, The Influence of Color on Apparent Weight, *Jour. Exper. Psych.*, 2, 1917, 347-370.

*Summary of Results*

(1) The apparent weight of color varies inversely with its brightness.

(2) *B* appears heavier than *R*, *R* heavier than *G*, and *G* heavier than *Y*. This order of apparent weight may be a function of the chroma, or of the inherent brightness of the chroma, or of both.

(3) There is a great variation between individuals in their judgments of the effect of saturation upon the apparent weight of color. In the majority of cases increased saturation increases the apparent buoyancy of the color and makes it appear lighter than the less saturated color.

(4) The relation of color to background affects the relative weights of colors differing in brightness. An increase in brightness of the background decreases the apparent heaviness of Dark *R* and increases the apparent heaviness of Dark *Y*. The background does not affect in any regular manner the apparent weight of colors differing in chroma or saturation.

(5) The factors involved in the apparent weight of color also influence judgments of aesthetic proportion.

(6) There is generally a close correlation between the direction of the judgments of the apparent weight of color and the judgments of aesthetic proportion. The greatest correspondence occurs between pairs of colors having the same chroma but different degrees of brightness and saturation. The least correspondence occurs between pairs of colors differing in chroma.

(7) In the majority of cases the values for aesthetic proportion are greater than the values for the apparent weight of color.

(8) Practice in judging the apparent weight of color increases the closeness of the correlation between judgments of apparent weight and judgments of aesthetic proportion and slightly decreases the individual variability of aesthetic judgments.

(9) The factors effective in the apparent weight of color also influence judgments involved in bisecting a line.

(10) There is a close correlation in direction between judgments of the apparent weight of color and judgments of the midpoint of the line. Judgments of the midpoint of the line are biassed toward the heavier color.

(11) The values for the apparent weight of color are much larger than the values for the midpoint of the line.

(12) The correlation between the apparent weight of color and the judgments of the midpoint of the line tends to decrease with practice.

# A STUDY OF THE EFFECT OF HYPNOSIS ON A CASE OF DISSOCIATION PRECIPITATED BY MIGRAINE[1]

By ROLAND C. TRAVIS
Psychological Clinic, State University of Iowa

We feel warranted in reporting cases of this type because of their unique contrast to, and radical deviation from, the normal tendency. But, as will be gleaned from the studies of Freud, Prince and Janet, the phenomena of dissociation, amnesias, somnambulisms and fugues are in the main exaggerations of normal tendencies. Every normal individual forgets names, experiences and events during the course of his life. At times he may even have slight amnesias, sleep-walking episodes, and periods of absentmindedness, all of which are miniature forms of the more exaggerated disorders such as complete dissociation, amnesias, fugues and somnambulisms. The significant difference between the normal and the abnormal in this connection seems to lie in the symptoms manifested rather than in the mechanisms leading up to those symptoms. The mechanism involved seems to be the common element in both normal and abnormal, and the difference to be one of degree rather than of kind.

*History.* The patient's family history is of little concern, except that his mother has had migraine for many years. The patient's father and mother lived together only a short time after his birth. When the patient was born he was nearly blind, and did not open his eyes until the eighth day. This blindness was caused by the mother's previous attack of smallpox. At the age of 7 the patient had the measles, and two weeks later be became paralyzed. He could not move his head, arms or legs, and at times lost his speech. He did not have much control of himself for a year after this attack. At about the age of 14 he was thrown from a load of hay, struck his head against a post, and fell to the ground; but he soon arose, ran out into the field, and seemed to lose all reason. He had to be held in bed the following night, and behaved queerly for two or three days. One year after this accident he wandered away from home, became homesick within a few days, and his mother sent him money to come home.

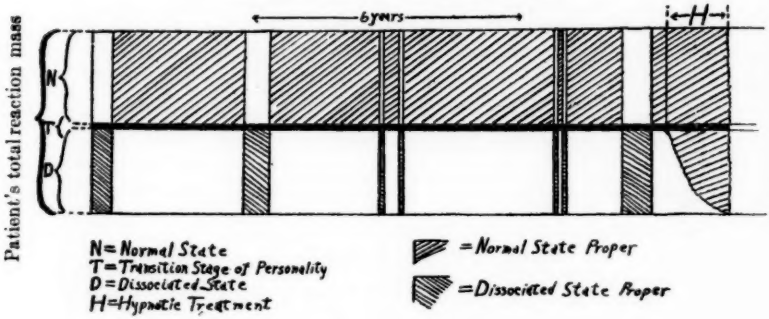
The patient recalls having had several sleep-walking episodes when he was about 13 years old. These episodes were not of the recurring type, but they are very vividly recalled. He has also had *pavor nocturnus* or night terrors in his childhood.

The patient enlisted in the army when he was 16; and after being transferred to two camps, he had an attack of hemiplegia or paralysis over the entire left side. The paralysis struck him while on the drill field; he suddenly fell to the ground in a sort of faint, and was unconscious for a short time. He could move none of the muscles of the left side and was entirely anaesthetic

[1]The case reported in this paper is a young man, twenty-two years of age who came to the State Psychopathic Hospital in a state of anxiety and worry over his recent episode of amnesia and dissociation.

over the same side except on the skin of his face and neck and a small portion of the upper abdomen. He was given good medical attention during the paralysis, which endured about two months. The medical officers decided to send him to Plattsburg, N. Y., for electrical treatments. Upon hearing this he began to improve immediately, and within a day or two his entire left side began to tingle and felt as though needles were pricking him. The tingling began in the ends of his toes and fingers and worked medially. Within two days after the initiation of the tingling he was able to walk around, although he felt rather stiff in his legs and arms.

Graph showing relative extent of the periods of dissociation in comparison with the normal periods between the attacks. The attacks extend over a period of six years




The patient has had 7 episodes of dissociation and amnesia extending over a period of 6 years with durations as follows: two months, three months, seven days, five days, eight days, three days, and four months. The accompanying graph shows the relative extent of the periods of dissociation in comparison with the normal periods between the attacks.

The first attack occurred when the patient had been transferred back to his home camp and was waiting to be discharged. He left the camp one day in uniform, and two months later came to himself in a distant city. How he arrived there, or why, he did not know. When he came to himself he had no money, and after finding out where he was he immediately telegraphed home for money. He remembers having had a severe headache before he left the camp and when he came back into the normal state in the distant city.

The second episode occurred while he was in the navy. He did as before; just wandered away from the camp with a severe headache, and after three months traveling, beating his way on trains, working at odd jobs, he awakened to find himself 800 miles from the naval station in civilian clothes. He had no money, and again telegraphed home for money. He was punished severely for having deserted from the navy, but gave no excuses for having left.

The next four episodes were of short duration, and occurred when the patient was at home and after his discharge from the navy. They show the characteristics of the other two with the exception that they were a great deal more confused.

The last or seventh episode of dissociation occurred after the patient had enlisted in the marine corps. He had worked up to the position of first class private, and apparently was enjoying his work immensely, when he was

suddenly struck with a severe headache. He went a few blocks to the "sick barracks" with his friend to get something to relieve his headache,—and this is the last he remembers in his normal state. He went from here to Washington and traveled from city to city in the eastern and central states, beating his way on trains, in autos, sometimes walking. He became an expert window cleaner, and this was his occupation when he remained for any length of time in a city. He finally came back to his normal state, after four months of wandering from city to city, in Windsor, Canada. He had a strange feeling that he was lost, and immediately oriented himself as to time and place and mailed a card to his mother saying that he would be home immediately.

After the patient's arrival at home he was forcibly impressed by the duration and possible consequences of his desertion from the marine corps. This was the condition in which he came to the hospital for treatment.

*Physical and Mental Examination.* The patient has always been an alert, energetic and athletic type of individual in his normal state. He has enjoyed good health since childhood with the exception of a terrific headache of irregularly persistent occurrence closely resembling migraine. The headache comes on instantly, and usually endures from 10 to 24 hours. It starts in the left frontal region of the head, and the pain shoots backward, at times becoming so intense that the patient says he cannot think. The facial artery throbs and is very much distended during the headache. The left eye and the left side of the head and the back of the neck feel sore after the headache has subsided. The patient sees occasional blotches and specks of light during the more intense part of the headache. Any kind or amount of medicine seems to have no effect on the headache. It runs its own course as far as the effects of drugs are concerned, again resembling migraine in this respect.

These headaches always accompany the transitional stages between the normal state and the state of dissociation. The headaches seem to act as a precipitant in the transition from one state to another. The headaches may occur and do occur, however, without the patient having any change in personality; but the transition from one state to the other never occurs without the accompanying headache.

The patient's physical examination showed nothing pathological. His spinal and blood Wasserman's tests were both negative. The X-ray of the head showed nothing abnormal. All reflexes were intact and normal.

The patient has had but an eighth-grade education, although his I. Q. was 106 on the Stanford Revision of the Binet Scale. This means that he has normal intellectual capacity, within the reliability of this test. He has a very definite *Wanderlust* characteristic, and has been over a great deal of territory for a man of his age. This trait is a great deal more pronounced during his state of dissociation, and he remained in one place but a short time,—a few weeks.

The patient passes from one state to the other seemingly in the midst of a terrific headache. He seems to come out of his dissociated state when he has run out of funds, when the weather is too cold for his slight clothing, when he is "up against it," so to speak. He seems to go into the dissociated state when he can stand no more of the domineering attitude of his officers, when he has a sudden impulse to see the world, when he wants to be free from all responsibilities.

When the transition from the dissociated state to the normal state takes place, he is very hungry, and has little or no money on his person. He describes his feelings as those of a person just waking from a dream. He feels lost and cannot realize where he is. He feels that he is in a strange country, and shouldn't be there. Then he makes haste to orientate himself, then

thinks of his mother and wonders if she is still alive. This prompts him to write home immediately, saying that he is coming home on the next train. He is ragged, and would pass very adequately for a tramp.

*Treatment.* The writer worked with the patient three weeks getting at the details of his history in regard to his physical and mental condition and his episodes of dissociation. In so far as his states of dissociation did not enter, to cause periodic and persistent blanks in his memory in his normal state, he recalled experiences as readily as any normal individual. But at the end of those three weeks the patient could recall nothing that had happened in the state of dissociation. He was becoming very much concerned over the whole matter. He saw more than ever before what such episodes meant to his well-being and success in life; but he could do nothing to prevent them; and he had, up to a few days before coming to the hospital, kept his amnesias, as such, a secret to himself, letting his relatives think what they might of his previous actions.

During these three weeks little or nothing was being accomplished by way of psychoanalytic attack. It was then decided to try hypnosis. After the third sitting in the hypnotic state the patient was able to recall almost in detail everything that had happened in his periods of dissociation even after he was awakened into the normal state. The results of the hypnotic treatments will be given in more detail.

The patient's willingness to submit to hypnosis was no doubt built up as the confidence increased between the writer and the patient during the three weeks of note taking. It required about 10 min. to get the patient under rather deep hypnosis. He was then asked to repeat everything that had happened to him in the past. He immediately told of his trip to Windsor; how he had come over on the ferry from Detroit that noon, that it was now 2 p.m., that he was now in Windsor, and that he expected to go back to Detroit Monday morning, for he had a job at cleaning windows in Detroit. He said he had a headache. He was asked to give his name, which he gave as Earl ———, an entirely different name from the one in his normal state. During this sitting and the next three sittings the major portion of the travelings and experiences of the patient during his states of dissociation were worked out and were connected up into a meaningful whole.

The hypnosis reinstated the dissociated state and brought the patient up to the last transition into his normal state. This is somewhat similar to the case in which Janet produced an artificial state by hypnosis in his patient Marceline.[1] Janet made Marceline think she was a well and happy girl again. But in the case under discussion, the patient went automatically, it seems, into the other state under hypnosis. The state could not be reproduced artificially, but could only be reinstated. While in the dissociated state induced by hypnosis, the patient could recall nothing that had happened in his normal state; and after the first hypnotic sitting he could recall nothing that was asked him or anything that he had said in the hypnotic state, or anything that had happened during his previous amnesic episodes.

A rather unusual and surprising discovery was made after the second hypnotic sitting. The patient could recall everything that had happened in his amnesic episodes and could connect it up with the rest of his previous experiences, but he could recall none of the questions asked him or the answers he gave. The process of hypnosis seemed to link the two states together, with the exception that the periods of transition from one state to the other were still a perfect blank whether in his normal state or under hypnosis.

In the third hypnotic sitting the patient was again in the dissociated state. He had a different name for each state, and while in the dissociated

[1] P. Janet, *Major Symptoms of Hysteria*, 1907, 86-92.

state could not recall his name of the normal state, his previous marriage, his life in the army and navy, his parents, his birthday (except that he was twenty *plus*), his home, any of his previous friends, or anything that had happened in his normal state. But he seemed to transfer the ability to make adjustments over into the dissociated state; for he beat his way on trains, worked at odd jobs, got into fights, and had normal sex-relations with women. While in the normal state he is a very good roller and ice skater, and can play football and many other athletic games; but in the dissociated state he cannot skate, play football, or any other game, can fight when he has to, and enjoys cleaning windows at great heights.

Nothing but a few more details was obtained in the fourth hypnotic sitting. In the fifth hypnotic sitting, the patient was hypnotized for the first time in his normal state. While under hypnosis this time he could recall nothing concerning the amnesic state. He was oriented as to time and place and could remember his experiences of his normal state. It was then suggested to the patient that he go into the other state and recall the experiences of the other state; this he did quite readily. It was then suggested that he concentrate upon the transition stage, especially the one previous to his last amnesic period. This occurred, it will be recalled, when the patient left the marine corps and went to Washington. It was tried very assiduously to get the patient to recall what had happened in several of the transitional periods, while he was hypnotized in the normal and amnesic states, but he could recall nothing. Everything was a perfect blank. It was repeatedly suggested that he would dream about the time that he went from the marine corps to Washington and that he would finally be able to tell everything that happened on that trip. (He was under hypnosis when these suggestions were made.) After about 10 min. of directed suggestion, the patient was asked to tell of his trip from the marine corps to Washington. He said he had no trip. He was asked "Where are you?" "I don't know." "Where were you yesterday?" "I don't know." "How do you feel?" "I have a headache." "What are you thinking about?" "Nothing—only my headache; it's hurting so bad I can't think." This was all that the patient could do in this state. Apparently he was in the transitional stage, and was totally disoriented as to time, person and place. His facial expression showed every evidence that he really had a severe headache. The patient was awakened directly into the normal state after having been in three states under hypnosis. He said he felt "all in" when he was brought out of the hypnosis. He was perspiring to excess, and felt as if he had done a hard day's work.

The patient had a headache for 2 hrs. immediately after the latter hypnotic sitting. This was probably caused by his being awakened directly from the transitional stage (the period when he has a very severe headache.)

The patient was somewhat resistant to being hypnotized again, because, he said, the effects of the last sitting gave him a headache; but he yielded to hypnosis after a little urging. This time it was possible to get the patient to recall all his experiences while under hypnosis, whether they were experiences of the normal state or of the state of dissociation. The transitional stages, however, still remained a blank. Several more hypnotic sittings revealed nothing in addition to the previous sittings, except that some of the details were ironed out more thoroughly.

In this case the experiences of the dissociated state have been properly connected up through a process of hypnosis with the experiences of the normal state. The transitional stages are still more or less a blank; but, from the facts obtainable on this period, the patient functions more nearly on a vegetative level, partially if not wholly unconscious of his surroundings.

The question arises whether the hypnosis has been adequate to the proper integration of experiences of the entire personality. Sidis and Goodhart[1] have said that "hypnosis alone is not sufficient to effect a synthesis of

two dissociated moments.[3] They quote James' description: "the waking state could give nothing else but Mr. Bourne's personality, the hypnotic could give only Mr. Brown's personality. By means of hypnotization, therefore, the two dissociated moments could not possibly be unified in one synthesis." In reference to the case under discussion, the above statement is partially if not wholly refuted, or else our case is a milder form of dissociation.

Prince also states, as a general principle, "that from a dissociated state to the full waking state, it is commonly found that there is amnesia for the previous state."[4] In connection with our case the hypnotic state itself was amnesic to the patient in the waking state, but through hypnosis the dissociated state was linked to the normal waking state to the extent that the patient's recollections of his experiences of the dissociated state were as vivid as those of his normal state. This case contradicts Prince's principle.

*Summary.* This is a report of a case of dissociation that might well be called a double personality. Treatment by hypnosis revealed the character of the second personality and its relation to the first personality, and some of the conditions necessary for the transition from the one state to the other.

In the first few hypnotic sittings the patient seemed to go automatically into the dissociated state. After repeated hypnotic suggestions the patient was able to recall all his experiences of both the normal and the dissociated states, either under hypnosis or in the normal waking state.

The transition-stage between the normal state and the dissociated state was always accompanied by a severe headache which closely resembled migraine in its symptomatology. The headache seemed to act as a precipitant in the transition from the one state to the other.

It was not only possible to hypnotize the patient in the normal state and in the dissociated state, but in the transition state as well. Here the patient seemed to be in a sort of stupor, thinking only of his headache, and totally unconscious of his surroundings. It was tried to superimpose other artificial states upon the personality of the patient while under hypnosis, but no sign whatever of a different state from the three already mentioned was elicited.

The following is a summary of the patient's attitudes in the three states. (1) The Normal State. The patient feels that he has seen enough of the world, and that he ought to get an education and make something of himself. He feels that he can work up to a responsible position quite rapidly, and has done so as a matter of fact. He had been a first class private in the marine corps, in charge of a boiler room in the navy, and assistant manager in a fibre company. He does not enjoy working at common laboring jobs, and especially abhors the window-cleaning pro-

[3] B. Sidis and S. P. Goodhart, *Multiple Personality*, 1905, 376-382.
[4] M. Prince, *The Unconscious*, 1914, 62.

fession, which he thinks he could not work into without some practice at the trade. He has always had an ambition to see the world, and likes immensely to travel. He likes to save his money although he has been quite a gambler in the army and navy. He is very fond of athletics, and is himself a very good athlete. He does not care much about women, but would rather be with men. He does not believe in taking chances as he did in the otherstate, that is, in getting out on a window ledge about five stories high. He enjoys going to church, and is affiliated with a part cular Church. He desires to have a home of his own with chilidren. He does not care for strong drink, although he has drunk somewhat with his gang. He likes the army and navy life very much. (2) The Dissociated State. The patient wants to be free from all responsibilities. He feels care-free and irresponsible. He feels perfectly at home in any strange city. He cannot take orders from other people, and says that he does not have to, because he can get along in this world without bending to other peoples' desires. He despises army and navy life because one has to take orders and cannot talk back to the officers, and one cannot do what one wants to. He is an expert window-cleaner and enjoys the work very much; he gets a great thrill in going out on ledges or in dangerous places. He likes to wander from place to place, and can remain in one place but a short time. He has an ambition to see the world, and would rather travel than do anything else. He feels quite contented when he is traveling. It does not matter how he travels, on the train, riding the blinds, on a freight train, or catching rides in autos. When he arrives in one town he wonders what is in the next town and must go there at once to satisfy his curiosity. He likes to drink quite well. He likes men as companions better than women. He cannot skate or play football. (3) The Transition State. One preponderant idea seems to dominate his consciousness, that he has a terrific headache. He thinks of nothing else. He is disoriented as to time, place and person. He appears to be on a stuporous vegetative level, unconscious of his surroundings.

When the patient was discharged from the hospital he was feeling quite elated over the fact that he could now connect all his previous experiences into a meaningful whole.

# AN EXPERIMENTAL STUDY OF MENTAL AND PHYSICAL FUNCTIONS IN THE NORMAL AND HYPNOTIC STATES[1]

By PAUL CAMPBELL YOUNG

*I. Historical and Critical Summary*[2]. Past experimentation in the field of hypnotism has been, on the whole, successful in establishing a symptomatology of this form of suggestion, *i. e.*, a description of what the hypnotized person *will do* in hypnosis that he *will not do* in normal consciousness. On the other hand, for lack of a strictly comparative method, it has not been highly successful in establishing the capability-differentia of hypnosis, *i. e.*, what the hypnotized person *can do* in hypnosis that he *can not do* in waking. The methods in the past have been of three kinds. (1) *Non-scientific investigation, i. e.*, investigation without definite control-conditions. With this method all things were possible[3]. (2) *Avowedly scientific investigation* of hypnosis as an isolated phenomenon, *i. e.*, with control-conditions in hypnosis, but without a comparable investigation of the same persons in ordinary waking consciousness. All 'scientific' investigation of this sort is vitiated by the fact that the normal powers have been grossly underestimated, and that the possibility always remains that the very persons who showed such remarkable powers in hypnosis could have done just as well in the normal state if they had been given to understand that fully as much was expected of them. In fact, if one begins to recite wonders because they are wonders, one need not go to hypnosis for even a thousandth of one's cases[4]. (3) *The comparative method, i. e.*, comparing the

[1]This paper is a condensation of a doctor's thesis presented at Harvard University in May, 1923. Because few readers will be interested in the mass of details connected with the investigation referred to in this paper, and because those who are so interested may consult the complete thesis in the Widener Library at Harvard, or may borrow a duplicate copy from that library, much matter which appears in the original thesis has been omitted from this article. At this time the author is interested mainly in setting forth the results of this investigation, which is, so far as he knows, the most extensive experimental study of hypnotism ever reported.

[2]A. Moll, *Hypnotism*, trans. from the 4th Ger. ed., 1909; J. Grasset, *L'hypnotisme et la suggestion*, 4th ed., 1916; L. Lowenfeld, *Hypnotismus und Medizin*, 1922. Any of these three books gives a good account of hypnotism *qua* hypnotism.

[3]Cf. Bourru et Burat: *Revue phil.*, 21, 1886, 311.

[4]For a feat almost as spectacular as that of Bergson's boy subject (*Revue phil.*, 22, 1886) see article to appear later on in regard to discriminating postage stamps in the normal as well as in the hypnotic state.

performance of *Ss* while they are hypnotized with the performance of the same *Ss* while they are in the normal state, under exactly similar conditions (except for the matter of hypnosis). This method has been consistently used up to the present time in only one series of experiments: that of N. C. Nicholson of Johns Hopkins University[5]. All other investigations which were meant to be strictly comparative in method are open to one or all of the following criticisms. (*a*) Often the comparison has been a desultory, not to say haphazard matter of a few trials in waking and a few trials in hypnosis, without any standardization of conditions. On the very face of them, most of the reports of comparative studies are worthless as scientific data. (*b*) There has been doubt as to what state of consciousness was being investigated. (*c*) The number and order of the experiments, the number and character of the persons acting as *Ss*, and the exact technique in the hypnotic and waking session have not been fully published along with the results. This criticism holds for the work of N. Ach[6], E. Trömner[7], A. Chojecki[8], and to an extent of all previous experimenters. (*d*) There have been too few *Ss*, often of a doubtful, neurotic kind; too few sessions; too limited a field of investigation. The studies of Claparède[9] and others suffer from these limitations. (*e*) The patent fact of wide variations in the successive performances of any given task by a normal *S*, even in waking life, has never been taken into account in interpreting the results of previous investigations[10]. It is a gratuitous assumption, in the face of all the possible factors of chance-bodily and mental states, *e. g.*, factors like mood, interest, tiredness, training under hypnosis, stage fright, etc., to select one factor, that of being awake or being hypnotized, as the cause of the differences (regardless of the direction in which the differences run!) which have been found in the two series of tests.

*II. Present Status of the Problem.* In the present research the following questions were dealt with. (1) Is there a real basis for the division of states of hypnosis into those that are active and those that are passive? Preliminary experimentation and the results of the 15 detailed experiments described below seek to answer this question. (2) Quite aside from the question of the ability to withstand fatigue, is there a difference in muscular capacity and nicety of adjustment? Experiments 1, 2 and 3

[5]Nicholson's work comes up later on in this article; 13 *infra*.

[6]Ueber geistige Leistungsfähigheit u.s.w., *Zeits.f. Hyp.*, 9, 1899, 1.

[7]*Journ. f. Psychol. u. Neur.*, 17, 1911, 307-433; 19, 1913, 286.

[8]*Arch. de Psy.*, 12, 1912, 61-67.

[9]E. Claparède et W. Baade, Recherches expérimentales, etc., *Arch. de Psy.*, 7, 1909, 300.

[10]For an example of wide variations see p. 221 *infra*.

bear upon this question. (3) Is sensitivity, *e. g.*, of the skin, greatly different in the two states? Experiment 4 deals in rough fashion with this problem. (4) Do the strictly mental functions involved in memory and perception vary consistently? This question received major attention in this research, and is taken up in all the experiments from No. 5 to No. 15, inclusive.

*III. Summary Statement of the Methods used in the Present Investigation. A. Plan of this Series of Experiments.* The purpose of this research was to put a large number of *Ss* through waking and hypnotic performances of the same simple tests, under as nearly identical conditions as possible, in order to determine the relative capacity of the same persons in the two states. The first requirement was to find suitable *Ss*; the second, to choose tests which should give comparable results in a relatively small number of sessions; the third, to establish such conditions and methods as would make for maximum performance in each state without being prejudicial to either; the fourth, so to treat the data obtained that the comparative performance would be apparent at a glance.

*B. Comparison of the Present Methods with those of Past Experiments.* The conditions of this research differed from those of previous ones in the following ways.

(1) In the number of *Ss* and their range in the hypnotic state.

(*a*) *Number.* Altogether 22 *Ss*, 16 hypnotic and 6 control *Ss*, took part in these experiments. On the average, there were 12.9 hypnotic and 3.2 control *Ss* for each of the 15 experiments. It is thus seen that all the *Ss* did not take part in every experiment.

(*b*) *Status.* The *Ss* were graduate and undergraduate students of Harvard University, and were palpably normal persons. Since they were all volunteer *Ss*, they were not dependent on the results of the experiments for any return, either financial gain or scholastic credit. Only one had ever been hypnotized before this year. None had been coached in the classical hypnotic mental attitudes or moulded in regard to matters like contracture, rapport, or post-hypnotic amnesia; hence they could furnish fresh evidence on these points.

(*c*) *Classification.* As a result of preliminary investigations, all those of the original *sixty* volunteers who had difficulty in attaining a rather stable depth of hypnosis were rejected. Those retained (except the 6 control *Ss*, who could not be hypnotized at all) were easily hypnotized, and each went into what, for him, was a characteristic depth of hypnosis. During the preliminary experimentation, using as a basis the *Ss*' behavior, the *Ss* were divided into the following classes:

(i) Six control *Ss*, who were not hypnotizable, but who went through the whole series of experiments twice, just as the hypnotized *Ss* did, once "awake" and once in "pseudo-hypnosis." During the pseudo-hypnotic session the control *Ss* received all the suggestions that put and kept the hypnotic *Ss* asleep, and were treated in every respect as though they had been hypnotized. (ii) Six light hypnotic *Ss*. (iii) Six deep hypnotic *Ss*.

(iv) Four somnambulistic *Ss*.[11] The deep hypnotic *Ss* were further classified into two groups: (1) those that were *alert*, active, and that would, if they went deeper yet, go into somnambulism, *i. e.*, into a clear-cut dissociation; (2) those that were *lethargic*, dull, and that would, if they went deeper still, fall into the most profound natural sleep. All the somnambulistic *Ss* are alert, however sleepy-eyed they look, and can be made to do, in hypnosis, almost anything that they can do in ordinary life, without any danger of waking up. The light hypnotic *Ss* do not go far enough in hypnosis to enable one to tell whether they are inclined to pass into somnambulism or into sleep.

Heretofore, there has been no comprehensive grading and classification of *Ss*, the number of whom, when specified, has varied from one to seven[12] *Ss*.

(2) In the use of control *Ss*.

So far as is known to the writer, this very ordinary precaution for any scientific experiment of this sort has never been taken before. As an objective check on the probability of variations from test to test with such a series of experiments and on the effect of suggestion, both matters quite apart from hypnosis, the presence of control *Ss* was indispensable.

(3) In the number of different kinds of tests used.

The advantages in using 15 different kinds of tests in this research are patent. Heretofore the most varied set of experiments was that of Claparède, with tests of memory for nonsense syllables, of adding, of reaction times, and of association times.

(4) In the use of sense material as contrasted both with nonsense and with rote material for testing memory.

All experiments made previously, that had measurable results, depended on either rote material or on nonsense syllables.

(5) In the number of sessions.

For the investigations reported in this paper there was a total of 252 sessions for the 22 *Ss*, 126 sessions in hypnosis and 126 sessions in the normal state. This is an average of 5.25 sessions for each *S* in each state of consciousness. In each of the 15 experiments (and in both parts of Experiment 10) there were on the average two sessions for the control *Ss*, and 2.4 sessions for the hypnotic *Ss* in each state. These figures are exclusive of the preliminary sessions (at least 3 with each *S*).

Although in individual tasks previous *Es* have reported even more sessions than were given to individual problems in the present research, the advantage in the present case is that the number of tasks and the number of experiments make for a large total of sessions.

(6) In the total time spent in experimenting.

In the present research 234 hours, 117 hours for each state, were spent in actual experimenting in obtaining the material of the 15 experiments. This amount of time is exclusive of time spent in preliminary sessions. All of

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[11]The light hypnotic *Ss* correspond fairly well to Wingfield's stages 1 and 2, the deep to his stages 3 and 4, the somnambulistic to his stages 5 and 6. In criticism of Wingfield it must be said that post-hypnotic amnesia has several grades, and cannot be used to characterize any stage of hypnosis unless it is specified what grade is meant.

[12]Braid, however, long ago spoke of two grades of hypnosis, and Trömner in 1910 contended that Chojecki had been experimenting in a sort of diffuse, twilight consciousness, resembling stupor, whereas he, Trömner, had been investigating a condition "mit partiellem systematischem Wachsein, nach Vogt".

this experimenting took place between November 1, 1922 and April 15, 1923.

Experiments reported heretofore have covered from one or two hours to what seems to have been several hours; but no such extended research as the present has been reported.

*C. Technique.* (1) Place and conditions of experimenting.

All sessions, of which there were an equal number in each state, were held in the same room in Emerson Hall, at corresponding hours for any one *S*. Although the experimental sessions for all *S*s were not of equal length, the sessions for any one *S* were equally long in the normal and the hypnotic states. The bodily position was always similar for the same task in the two states. Lights, furniture, apparatus, etc., were kept uniform as regards the same individual experiment in the two states.

(2) Temporal order of the experiments, of sessions, and of waking and hypnotic periods.

For detailed treatment of this matter one must consult the original thesis. In any individual session, as well as from session to session, the experiments were taken up in the following order: 1, 7, 5, 10, 6, 9, 8, 4, 11, 12, 14, 13, 2, 3, 15.

The trials in hypnosis and in waking always alternated as follows, whether the sessions were single, *i. e.*, sessions in which only one state of consciousness was tested, or double, *i. e.*, sessions in which both states of consciousness were tested at one sitting: the first trial was a waking trial, the second an hypnotic, the third an hypnotic, the fourth a waking, etc.

(3) Method of hypnotizing.

The author did his own hypnotizing, by the method of fixation of the eyes and verbal suggestion, without manipulation.

(4) Apparatus and test materials.

No intricate apparatus was used. Samples of the test materials accompany the original thesis. Materials were either identical for the two states, or were so constructed (*e. g.*, lists of words composed in pairs) and so modified when necessary—as shown by several trials on persons not taking part in these experiments—as to be of equivalent value. Besides, in the case of paired lists for testing memory, one list was given to half the *S*s in the waking state and to half in the hypnotic state. In this way, if, after all, one list was more difficult than its companion, the effect would be shared by both states.

(5) Preliminary experimentation.

To prevent the appearance in the published results of the effects of novelty and stage fright, and the unequal influence of the *S*s' being "easy" in hypnosis and uneasy in waking (for in hypnosis everything that happens seems quite natural), 3 preliminary sessions, made up of both hypnotic and waking periods, were held; that is, 3 for each *S*.

(6) Keeping constant the state of hypnosis.

Although the original thesis gives details on this technical point, it should be noted that this matter was simplified by the rejection, during the preliminary sessions, of those candidates who could not achieve a stable condition of hypnosis.

(7) Instructions.

The instructions accompanying each test were purposely made identical for the two states. The highest possible performance was demanded at all times.

### (8) Making observations and recording results.

The *E* read off the test materials and took down the simple records necessary in each experiment.

### (9) Treatment of the data.

Those interested can consult, in the thesis, Tables for each of the 15 experiments, and, besides, 3 Tables of summaries. Here, however, only two Tables are given. These deal with the averages of the various classes of *Ss*. Explanations accompany the Tables. Since not the actual accomplishment of the *Ss* but the relation of their hypnotic performance to their waking performance is of importance in this study, the emphasis has been placed on the hypnotic-normal ratio for each experiment. This ratio is in every case the *percentage* that the *S*'s hypnotic score is of his waking score; that is, the average score made in the waking session in these experiments is assumed to be 100%. For example, if the ratio 90 appears in a Table, it means that the hypnotic performance is 10% less than the normal; if the ratio is 120, the hypnotic performance is 20% better than the normal. So, then, in this report the actual results do not appear; what is given is the ratio obtained by dividing the hypnotic performance by the normal performance, expressed in percentage.

## *IV. Experimentation.*

*Experiment 1: Movement of the Arm over a Sector*

*A. Procedure.* The purpose of this experiment was to compare the accuracy in waking and hypnosis of moving the right hand to a mark on a sector, with the eyes closed. The materials consisted merely of a sector of 120° drawn in ink on a piece of beaver board, which was tacked securely to a table in such a manner that a uniformly smooth surface was within reach of the *S*'s hand, with zero at the left. The movements, all of which started from zero, and were consequently only in one direction, were of two kinds: single movements to a number, and movements reproducing such single movements. At each session, in both states of consciousness, the *Ss* were oriented to the sector by making a set number of moves, 6 in all, which were guided by the *E*. The *S*, of course, sat at the table.

This experiment represents for each of the 18 *Ss* who performed it either 3 or 4 hours of experimentation, according as it was gone through twice or three times in each state. The total time was 62 hours, half of which was in the normal state.

The record consisted in keeping account of the total amount of errors (in degrees) of the movements in each state, computing the average deviations of the errors, totalling separately for each state the amounts by which the *S* exceeded the mark chosen (overestimation), and the amounts by which he fell short of the mark (underestimation). These data were then reduced to a hypnotic-waking ratio, which clearly shows the relative ability in the two states.

*B. Results.* In general, the results show that 11 of the 12 hypnotic *Ss* made more errors and 10 of the 12 had a greater mean deviation of error in the hypnotic state than in the waking state; whereas there was no appreciable difference in the accomplishments of the control *Ss* in the two states. With the hypnotic *Ss* there was a tendency in waking to overestimate rather than to underestimate by the ratio of 308° to 109°, and in hypnosis by the ratio of 838° to 0°. The word "ratio" here is not used in the sense described in the preceding Section. The control *Ss*, in contrast with the hypnotic *Ss*, did not overdo the matter of overestimation or underestimation in either state, although the amount of overestimation in both states (waking and pseudo-hypnosis) was greater than the amount of underestimation: 286° (o.e.) and 195° (u.e.), 182° (o.e.) and 152° (u.e.), respectively for waking and pseudo-hypnosis.

As for ratios (in the sense of the preceding Section), the control *Ss* have 104% as regards total errors (symbolized by t.e.), and 97% as regards the mean deviation of errors (m.d.); the light hypnotic have 89% t.e., and 86% m.d.; the one deep alert 89% and 91% resp.; the two deep lethargic 69% and 79% resp.; the somnambulistic 79% and 82%, resp. The final ratio of all hypnotic *Ss* is 79% for accuracy of the movements and 84% for consistency.

*C. Discussion of the Results.* Of the 15 experiments reported in the present article, this is the only one in which there is a significant and consistent difference between the capacity in waking and in hypnosis. The explanation of this lowered performance and tendency to overestimate in hypnosis on the part of the hypnotic *Ss* seems plain enough. Since the suggestions of easiness and relaxation have been accepted by the *Ss*, there is an inclination to freedom and largeness of movement. Whatever suggestions as to taking care and moving exactly to a number are superadded can merely restrain to some degree this tendency. The fact that the mean deviations are almost one-fifth greater in hypnosis shows that the keeping of a definite image of the board in mind was not quite so good in that state as in waking. That mere lackadaisicalness accounts for the difference is hard to believe, because the somnambulistic *Ss*, who were fully as intent on the task in hypnosis as in waking, showed the same tendencies very markedly.

*Experiment 2: Dynamometer Test*

*A. Procedure.* The apparatus for this test was Salter's improved dynamometer, registering 112 pounds by half-pounds. The experiment consisted of obtaining at each session 5 scores for each hand at gripping the apparatus. Thus 10 scores were obtained at each session. For the 14 *Ss* taking this test there were 616 scores in all, half of which were secured in the normal state. In hypnosis the *Ss'* eyes were closed, in waking they were kept open; otherwise the conditions were the same for the two states. The instructions, to grip the apparatus as tightly as possible, were repeated before each of the 10 trials at each of the sessions, in both states. In neither state was the *S* informed of his scores.

*B. Results.* This experiment, which measures only the ordinary capacity, not the capacity of resisting fatigue, shows what will become an old story in this paper; a lack of significant difference in capacity between the two states. The record shows that in hypnosis 7 hypnotic *Ss* did better than in waking, 4 did worse, and 1 equalled his record; whereas the three control *Ss* were scattered between a better, an equal, and a worse performance in pseudo-hypnotism than in waking. As far as average ratios go, the control *Ss* did 1% better in pseudo-hypnotism; the light, 4% better; the deep alert, 4% worse; the deep lethargic, 2% worse; the somnambulistic did equally as well in hypnotism as in waking. The average ratio of all hypnotic *Ss* is just 100%, as against 101% for the controls.

These results seem to indicate that the capacity of the muscles brought into play is about equal in the two states. Whether with continued trials (which would bring in question the power of resisting fatigue) the results would be similar to those in this test, is an entirely different matter, and one which Nicholson[13] seems to have settled in the negative, by obtaining results which were greatly in favor of the hypnotic performance at working at an ergograph.

*Experiment 3: Steadiness Test*

*A. Procedure.* The apparatus for the test was the tracing board with an ever narrowing groove[14]. At each session there were two preliminary

[13] *Johns Hopkins Hospital Bulletin*, No. 31, 1920, 89-91.
[14] G. M. Whipple, *Manual of the Simple Processes*, 152, Fig. 32.

movements, and 10 successive trials of continuous movement down the groove; and besides, 25 trials of continuous jabbing the stylus between the sides at the 2-mm. point. The final ratio was obtained by adding to the ratios of the jabbing, twice the ratios of the continuous movement and dividing by three, thus weighting the scores of the continuous movement. In all, there were 2,156 scores, made by 3 control and 11 hypnotic *Ss*, half of the scores, as usual, being obtained in the hypnotic state.

*B. Results.* Two of the three control *Ss* are distinctly poorer in hypnosis; the third is distinctly better. The ratios are 45%, 73%, 134%[15]. Seven of the 11 hypnotic *Ss* were better; the final average ratio is 109%. The light hypnotic *Ss* vary greatly, with ratios of 105%, 125% and 73%, an average of 101%. All the deep hypnotic *Ss* of both types give results clearly in favor of hypnosis, the average ratio being 132%. The somnambulistic *Ss* have ratios of 109%, 109%, 76% and 75%, an average of 92%. If this small, possibly chance, difference between the capacity in waking and in hypnosis needs explanation, it may lie in the fact that this experiment as conducted bordered slightly on a fatigue experiment, and that in hypnosis the subjective factor of fatigue was in abeyance. The low score of the controls can be explained by chance, on the basis of the small number of *Ss*, only 3 as against 11 for the hypnotic *Ss*.

*Experiment 4: Pressure of Two Hairs on the Skin*

*A. Procedure.* For this experiment one hair of 15/100 mm. diam. and 570 mgm. force, and another of 1/10 mm. and 220 mgm. were used. The method was that of areal stimulation of that portion of each hand lying between the knuckles of the index and middle fingers. There were for each of the 15 *Ss* an average of 756 stimulations, 378 in each state, making a total of 6,670 stimulations in each state of consciousness. The hairs were glued to skewers and applied by hand. In regard to the order of stimulations, precautions against fatigue, secondary cues, and prejudicial suggestions, the original thesis must be consulted.

The instructions in this experiment were the same in both states as far as the orders to pay attention and answer "yes" every time a touch was felt were concerned. They differed in the fact that in hypnosis the hand was "sensitized" by suggestions of greatly increased ability to feel the slightest touch, and by having the *S* report that he did feel this increased sensitivity in himself. Getting the hypnotized *S* to believe that his hands are extremely sensitive is one of the easiest tasks a hypnotist can perform. Every hypnotic *S* reported greatly increased sensitivity, and often reacted quite strongly until he was ordered to feel the touch only as *pleasantly* sensitive.

In computing the score the total number of correct responses given at the stimulation of the stronger hair was added to twice the number of reports at the stimulation of the weaker hair. In this way the ability to feel the slighter pressure was weighted in the final score.

*B. Results.* The two control *Ss* differed greatly in this test, the ratios being 85% and 121%[16], with an average of 103%; the four light *Ss* were equally divided, showing ratios of 103, 89, 117 and 86, an average of 99%; both deep alert *Ss* were poorer in hypnosis, with ratios of 91 and 97; the three deep lethargic were much poorer, with ratios of 91, 72 and 69, and an average of 77; the somnambulistic, however, came back up the scale with scores of 97, 108, 95, and 101, an average of 100.

The average of all hypnotic *Ss* was 93%, as against 105% for the control. Not much stock can be taken in this comparison, however, because of the

[15]An instance of very great chance differences, since the matter of hypnosis did not enter at all.

[16]Another case of wide variation within one state of consciousness; here, again, there was no hypnosis at all.

small number of control *Ss* and the small number of trials made with them, 896 altogether, in comparison with that of the other class, 10,444 altogether.

*C. Discussion.* That there is in hypnosis a very great subjective intensification of the consciousness in regard to a touch which is actually felt, there can be no doubt. That this subjectively increased sensitivity may exist along with an actual lowering of objective sensitivity seems probable. Judging from the results of this test, it is in an experiment such as this, more than in any other kind, that one would look for Braid's two grades of hypnosis: one bringing about greater sensitivity, the other lowering it. Even though the present series of experiments is far from verifying any increased sensitivity of the skin, it may justly be said to indicate two types of hypnosis: (1) the type in which the *S* becomes less sensitive to touch, and (2) the type in which, for all one knows (judging by the combined results of the deep alert and of the somnambulistic *Ss*), the *S* retains his ordinary sensitivity.

It is well to note, however, that this division is not on the basis of depth of hypnosis—as has been erroneously thought—but on the basis of the different qualities of the states induced. The results seem to indicate that the very deep-going type, *i. e.*, the somnambulistic *S*, shows the most nearly *normal* sensitivity.

*Experiment 5: Spelling Backward*

*A. Procedure.* After telling the *S* to listen carefully to some words which he was to spell backwards "as fast as you can, still being accurate," the *E* gave in turn each of the following couples: "Shakespearean criticism"; "Evolutionary hypothesis"; "Experimental psychology," and "Philadelphia, Pennsylvania." The same words were used in all the sessions, but the order was varied, so that if today "Shakespearean criticism" came first, next week it would come last.

From the test two sets of scores were obtained, one in sec. and one in number of mistakes. No attempt was made to reduce them to a unit. There were, on the average, 2.3 sessions in each state for the 3 control *Ss*, and 2.2 sessions in each state for the 16 hypnotic *Ss* participating in the experiment.

*B. Results.* In speed only one of the 3 control and only 5 of the 16 hypnotic *Ss* did better in hypnosis than in waking; the average ratios are 105 and 95 for control and hypnotic, respectively. The control *Ss*, however, made up for their increased speed by a greater number of errors, the average being 17 errors in waking and 21 in pseudo-hypnosis. The hypnotic *Ss*, on the other hand, had an average of 12 mistakes in each state.

Among the hypnotic *Ss* the ratios for speed were as follows: light, 94; deep, 100; somnambulistic, 89. The somnambulistic, with the exception of one *S*, who made a ratio of 100, showed less speed than in the normal state, obtaining ratios of 90, 84, and 81; while they were furnishing only one less error.

If the hypnotic state brings on any great increase in the power of visualization, the scores in this test should have shown a marked betterment in hypnosis; unless, of course, this increased power of imaging should be offset by a decreased power or will to seize the image and "read it off." It seems that it is just that tendency to forge ahead, even at a task commanded ever so explicitly, which is lacking in hypnosis.

*Experiment 6: Saying the Alphabet Backward*

*A. Procedure.* This simple test was given with the same instructions as Experiment 5, viz., "Go as fast as you can, still being accurate." The record, made by the *E*, consisted in copying down all letters spoken by the *S* and noting the number of sec. spent in the process. Sixteen hypnotic *Ss* and 3 control *Ss* with an average of 2 sessions each in each state took part in this test.

*B. Results.* Although, on the face of it, this test closely resembles Experiment 5, the results are somewhat different. Here, 2 of the 3 control, and 13 of the 16 hypnotic *Ss* showed a better record in the relaxed state: with ratios of 106 and 108 for control and hypnotic *Ss* respectively. The average errors were: for control, 2.3 errors in waking to 1.6 in pseudo-hypnosis; for hypnotic, 2.5 errors in waking to 2.3 errors in hypnosis.

The explanation of this higher performance in saying the alphabet, unless it be a mere matter of chance (an alternative not to be lightly dismissed) may be found in the fact that a certain amount of initiative is required in spelling words backwards; whereas with a few repetitions of the alphabet backward a habit is formed. The more mechanical saying of the alphabet may not disturb the hypnotic relaxation; whereas spelling backward brings up a conflict between remaining easy and exerting oneself.

*Experiment 7: Digit Span*

*Results.* Records were obtained on 3 control *Ss*, with an average of 2.0 sessions in each state, and 16 hypnotic *Ss*, with an average of 2.5 sessions in each state, in repeating digits which had been read aloud. Although in hypnosis two of the deep lethargic *Ss* simply could not keep the digits in mind, and the rest of the hypnotic *Ss* reported great subjective certainty (for one *S* the digits were "engraved in granite," albeit sometimes the wrong digits; another "read them from a blackboard"), still, 12 of the 16 hypnotic *Ss* equalled or bettered their score, with a final average of 101. The control had a ratio of 106. The conclusion is that there is no difference in this type of rote memory of these *Ss* in the two states. Although the somnambulistic *Ss* did 5% better in hypnosis, the control *Ss*, without being hypnotized, did 6% better in pseudo-hypnosis.

*Experiment 8: Cumulative Adding*

*Results.* This series of adding consisted in the *S*'s saying aloud the number read to him by the *E*, *e. g.*, 9, then adding each time a greater number by one to the sum until he had added the number 12, as follows: 10, 12, 15, 19, etc. Only the sums were said aloud by the *S*. The record, which was just as extensive as the preceding one in respect to *Ss* and sessions, consisted of all the numbers spoken and of the time consumed (sec.). All 3 control *Ss* increased their speed, but made more errors in the pseudo-hypnotic state; 8 of the hypnotic *Ss* bettered both their speed and their accuracy in hypnosis; whereas 1 hypnotic *S* did equally well, and 7 did poorer. The final ratios for control and hypnotic *Ss* are respectively 110 and 102. We thus see that the behavior of the control *Ss* in the pseudo-hypnotic state was less "normal" than that of the hypnotic *Ss* in hypnosis itself.

*Experiment 9: Memory for Nonsense Syllables*

*A. Procedure.* These tests were conducted on the basis of retained members, trial being made after 5 readings. After the various series, of 10 couples, had been carefully prepared with a view to making them of equal difficulty, they were divided into two classes, one class for half the *Ss* in waking and half the *Ss* in hypnosis; the other class for the remainder of the *Ss*.

In all, 3 control subjects with an average of 2 sessions in each state, and 16 hypnotic *Ss* with an average of 2.4 sessions in each state, performed this experiment.

*B. Results.* It is of interest that in this test there is a steady loss of performance as depth of relaxation increases until the somnambulistic *Ss* are reached; with the somnambulistic, however, there is such an improvement as can hardly be laid to chance. The ratios are: control 98, light 91, deep lethargic 78, deep alert 71, somnambulistic 115. The individual somnambulistic *Ss* have ratios of 97, 105, 135, 121. The ratio of all hypnotic *Ss* is 91.

*C. Discussion.* The results seem to show that the monotonous reading of nonsense syllables, in the three lightest forms of hypnosis, overcame the

explicit direction to note carefully and to remember. The performance and the introspections of the somnambulistic *Ss* lead the *E* to believe that associations of a rather fantastic, irresponsible kind are easily formed in the somnambulistic state.

*Experiment 10: Memory for Adjective-Noun Associations*

*A. Learning: Procedure.* The method was that of reading through once a series of 25 adjectives linked with nouns, and then, on the basis of retained members, getting a score for learning. This procedure was repeated until all the members were learned. The scores from successive readings were so weighted that, the fewer the readings necessary, the higher was the score. Scores were obtained for 4 control *Ss* with an average of 2.2 sessions in each state and 15 hypnotic *Ss* with an average of 3 sessions in each state.

*Results of Series A.* The average of all control *Ss* shows a ratio of 96%; of all hypnotic, a ratio of 98%. That is, both types of *Ss* were a little poorer when in the relaxed state than when in the normal state. However, the hypnotic varied as follows: light, 102; deep alert, 83; deep lethargic, 89; somanmbulistic, 109. The control *Ss*, moreover, differed among themselves greatly, with ratios of 96, 143, 75, 71, as did also the somnambulistic, with ratios of 97, 136, 99, 102.

*B. Retention.* By giving only the first number of the couples learned at previous sessions, scores for retention were obtained. (Eight distinct sets of 25 associations each were used in Experiment 10.) The control *Ss* did better in pseudo-hypnosis, having a ratio of 107%. The hypnotic *Ss*, likewise, made a better score in hypnosis, showing a ratio of 105%. The hypnotic classes varied widely: light, 101%; deep alert, 101; deep lethargic, 89; somnambulistic, 116. In the control group the individual ratios were 102, 170, 62, 95.

*Discussion of Results of Experiment 10.* One might say that the results of this experiment show either chance differences or individual differences. If they show individual differences, some *Ss* tend to do better in the relaxed state, others worse. That the differences cannot be laid to hypnosis *qua* hypnosis is apparent; since the control *Ss*, without being hypnotized at all, manifest the same kind of differences as do the hypnotic *Ss*.

*Experiment 11: Memory Span*

Immediately after each reading of various lists of 10 names each (philosophers, poets, etc.), trial was made to see how many the *S* could repeat aloud. This procedure was followed until all 10 items were remembered.

The idea of hypnosis as a state in which the *S*'s mind is a *tabula rasa*, which may be written on in indelible ink, receives no confirmation from this test. The light hypnotic *Ss* have an average ratio of 105, the deep 84, the somnambulistic 98. Only 9 hypnotic *Ss*, and no control *Ss*, took this test. The final average for all hypnotic *Ss* is 96%.

*Experiment 12: Memory of Miscellaneous Associations*

The methods of giving this test and of computing the score were the same as those of Experiment 10. Here, however, only scores of learning were obtained. The materials consisted of unrelated words read aloud in couples. Two control *Ss* with an average of 2.5 sessions in each state, and 11 hypnotic *Ss* with an average of 2.5 sessions in each state took part.

This test, which surpasses most of the past experimentation in extent of time and the number of *Ss*, gives results from which one may rightly generalize. For example, the two control *Ss*, with hypnosis out of account, made ratios of 111 and 167, an average of 139; whereas the hypnotic made a ratio of 97, with individual scores running from 78 to 130. The conclusion is not that two normal states (as shown by the scores of the control *Ss*) differ more than a normal state does from an hypnotic, but that chance differences

and individual differences may account for the wonderful reports of increased powers in hypnosis, with which the literature abounds. With a few *Ss*, in a few experiments, one *E* gets one set of results; another gets quite contrary results. With a relatively large number of *Ss*, in several sets of experiments, the chance and individual factors tend to be averaged away. In this very experiment the somnambulistic *Ss* had individual ratios of 110, 113 80, and 78.

*Experiment 13: Logical Associations*

*A. Procedure.* Following a suggestion given by Professor McDougall, the *E* made lists of 20 words in logical sequence, the words leading from one to the next in such a fashion that they could be easily remembered as a whole; *e. g.*, Sir Galahad, knight, tournament, tennis, ball, etc. After reading the list of 20 words, the *E* gave the first word, and allowed 2 sec. for the *S* to produce each of the following words in order. If within that time no word, or a word not appearing in the list, was given, the correct word was said by the *E*, and the *S* was charged with a full mistake. If within the 2 sec. the wrong word, but a word appearing in the list, was given, the correct word was supplied by the *E* and the *S* was charged with half a mistake. Without further readings of that list, the *S* continued to repeat the list until he could go through it without making a mistake.

Three control *Ss*, with an average of 2.0 sessions in each state, and 11 hypnotic *Ss*, with an average of 2.8 sessions in each state, performed this experiment.

*B. Results.* Two of the 3 control *Ss* were poorer in the relaxed state, the ratios being 96, 90, and 106, an average of 97%. Seven of the 11 hypnotic *Ss* bettered their score while in hypnosis, the final average being 99%. The range of hypnotic scores is shown by ratios of 85 to 114. The somnambulistic show individual ratios of 98, 94, 114 and 101.

*C. Discussion.* In this experiment and in Experiment 10, if anywhere in this research, one should have expected to find a decided advantage in favor of the hypnotic performance. Since not the *S*'s own report of his memory, but his actual performance in remembering was taken, this test would have been almost final, if consistent differences between the normal and hypnotic abilities had been found. That no such differences were found leads one to conclude that in bettering *present* memory[17], either in respect to learning or to retention, hypnosis can do little or nothing.

*Experiment 14: Free Association*

Thirty sec. was allowed for each of the 2 trials given at each session in both states in saying as fast as possible the words that came to mind. The *E* gave a common word, such as "city," for a stimulus at each trial.

The 3 control *Ss*, with an average of 2.0 sessions in each state, showed no consistent improvement in the relaxed state, the final ratio being 98%. The 11 hypnotic *Ss*, with an average of 2.3 sessions in each state, ranged from 77% to 128%, with an average ratio of exactly 100% (a marvel of chance to get so round a number!). Five of the hypnotic *Ss* did as well or better in hypnosis than in waking.

Since saying the words aloud required bodily exertion, and was, to some extent, contrary to the general suggestions of relaxation, there may be in hypnosis a very slightly increased power of association in one direction. It is not clear whether this is a case of the so-called narrowed consciousness (*i. e.*, an inhibition of all activities in the brain but the one in play at the moment); or whether it is quite the reverse: a diffusion of the energy of the brain, so that whatever activity is in play may find an outlet through any number of channels.

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[17] A later paper will seek to show that, for some *Ss*, hypnosis brings about a revival of long-forgotten memories.

*Experiment 15: Discrimination of Ink Blots*

*A. Procedure.* Under Professor McDougall's direction the *E* formed 26 ink blots on 26 regular visiting cards, in such a way that several of the figures resembled each other to some extent, and yet had differences which, with anything like close study of the individual card for 2 sec., could be easily distinguished. Before the cards were used, they were divided into two sets (by trial on persons not in the experiment) of about equal difficulty as far as discrimination went. The technique was as follows: after telling the *S* that he was to be shown a set of cards each of which he was to study very carefully so as to be able to pick it out later, the *E* let the *S* look at each of the 13 cards under a good desk-light for two full sec. Then the cards were shuffled with the other set, and the *S* was asked to say at the showing of each of the 26 cards, whether he had just studied it or not. A simple "yes" or "no" was all that was required. The score for each trial was computed by adding up all the wrong choices and subtracting the sum from 26. Two such scores were obtained at each session in both states for 3 control and 11 hypnotic *Ss*, with 2.0 sessions in each state for every *S*.

*B. Results.* The scores in the relaxed state, in this test, more nearly approximate the normal scores than in any other of the 15 experiments. The average ratios of both the control and the hypnotic *Ss* are exactly 100%. The 3 light *Ss* give an average of 102; the deep alert give 92; the deep lethargic 96; the somnambulistic 104. The numerical distribution shows one control doing better, one worse, and one equally well in hypnosis; 5 hypnotic *Ss* doing better, 3 worse, and 3 equally well.

The showing made in this test of discrimination, with the results both measurable and comparable, should have some weight in establishing the fact that normal persons of well developed powers can receive from hypnosis little, if anything, by way of augmented powers.

*V. Summary of the Results of the Fifteen Experiments.* Some of the facts summarized in this section may be studied in the two Tables printed herewith; for other facts, the thesis itself must be consulted.

The summary can be best set forth under the heads of (1) results according to *Ss*, (2) results according to tests, and (3) results according to the physical or mental nature of the experiment.

(1) *Results according to Ss.* (*a*) *Control Ss.* Table 21 of the original thesis shows that in the average of 8.66 tasks in which the 6 control *Ss* participated (from other Tables it is plain that all *Ss* did not take all tests) they made a better score in pseudo-hypnosis than in "waking" in 4 experiments, the same score in .5 experiments, and a poorer score in 4.16 experiments. The figures just given are marked "a" in Table 21, and represent the score of individuals in single tests (not averaging the scores of individual *Ss*). On the basis of averaging the scores (*i.e.*, the ratios) of each of the *Ss* in one experiment, the results were better in 9 tasks, the same in 1 task, and worse in 6 tasks. Taking into consideration both of these sets of figures, but without trying to reduce them to a unit, one concludes that in the experiments reported above the control *Ss* were on the average slightly better in pseudo-hypnosis, *i. e.*, in the relaxed state in which they

had received the same kind of suggestions as had induced hypnosis in the other *Ss*, than they were in the ordinary state. This fact may be seen by reference to the Table published in this paper, Summary I; the final ratio is 103%.

(*b*) *Hypnotic Ss.* By referring to Table 21 of the original thesis, one sees that on the basis of the individual *S*'s score in each test (not averaging the scores) the 16 hypnotic *Ss*, participating in 13.7 tasks on the average, did better in 5.875 tasks, did equally well in .875 tasks, and worse in 6.9 tasks. On the basis of averaging the scores of the 16 *Ss* in each of the experiments, the hypnotic *Ss* in hypnosis did better in 3 tasks, equally well in 3 tasks, and worse in 9 tasks. From this summary of the results one must conclude that, for these experiments as a whole, performance of all hypnotic *Ss* in hypnosis was slightly lower than in the normal state. This conclusion is borne out by the final ratio (shown in Summary I in this paper), which is 97%.

It is important, however, to notice this fact: if the results of the first set of experiments (movement of the arm over a sector) are not averaged in with the other experiments, the grand summary of all hypnotic *Ss* is exactly 100%, a remarkable example of the efficacy of having a large number of *Ss* and sessions.

The reader's attention is called to the fact that there is less difference, on the average, between the performance of the hypnotic *Ss* in hypnosis and their performance in waking consciousness than there is between the performance of the control *Ss* in the two states of normal consciousness (*i. e.*, the "waking" and "pseudo-hypnotic"). That the differences in the performances of the hypnotic *Ss* in any given test, or of any individual *S* in one or all tests, are due to hypnosis *per se*, is a gratuitous assumption. Experiment 1, Accuracy of Movement, provides the only exception to this sweeping statement.

A summary of the classes of hypnotic *Ss* follows. (1) The light hypnotic *Ss* did better in hypnosis in 6.33 tasks, equally well in .66, and poorer in 6.0. On the basis of averaging the scores of all *Ss* in a given task, however, they did better in 12 and worse in 5 tasks. The conclusion that they were slightly better in hypnosis than in waking should be made, even though Summary I of this paper shows their final ratio as exactly 100%. (2) The deep alert *Ss* were better in hypnosis in 5.0 tasks, equal in .5, and worse in 9.5; whereas the deep lethargic were better in 4.33, equal in .66, and poorer in 7.66. In the scores obtained by averaging the performance of the *Ss* in a given test the results are: for the deep lethargic, 3 with better scores, 1 with equal, and 13 with poorer scores; for the deep alert, 4 with better, 1 with equal, and 12 with poorer scores. Summary I in this paper gives ratios of 93 for the deep lethargic and 95 for the deep alert, as final ratios. (3) The somnambulistic *Ss*, on the basis of tak-

SUMMARY I

| *Subjects* | Exp. 1 | Exp. 2 | Exp. 3 | Exp. 4 | Exp. 5 | Exp. 6 | Exp. 7 | Exp. 8 | Exp. 9 | Exp. 10 | Exp. 11 | Exp. 12 | Exp. 13 | Exp. 14 | Exp. 15 | Avg. All Exper. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Light Hyp. | A81 B86 | 104 | 101 | 99 | 94 | 111 | 103 | 102 | 91 | A102 B101 | 105 | 104 | 102 | 106 | 102 | 100 |
| Deep Hyp. A. Alert type | A89 B91 | 96 | 128 | 94 | 100 | 125 | 104 | 106 | 71 | A83 B101 | 75 | 95 | 88 | 84 | 92 | 95 |
| B. Lethargic type | A69 B79 | 98 | 137 | 70 | 100 | 105 | 92 | 87 | 78 | A89 B89 | 89 | 93 | 97 | 106 | 95 | 93 |
| Avg. of All Deep Subjects | A76 B83 | 97 | 132 | 84 | 100 | 112 | 96 | 101 | 75 | A86 B93 | 84 | 94 | 93 | 95 | 96 | 94 |
| Somnambulistic | A79 B82 | 100 | 92 | 100 | 89 | 100 | 105 | 104 | 115 | A109 B116 | 98 | 96 | 102 | 99 | 104 | 99 |
| Avg. of All Hyp. Subjects | A79 B84 | 100 | 109 | 93 | 95 | 108 | 101 | 102 | 91 | A92 B104 | 96 | 97 | 99 | 100 | 100 | 97 |
| Avg. of All Control Subjects | A104 B97 | 101 | 84 | 103 | 105 | 106 | 106 | 110 | 98 | A96 B107 | — | 139 | 97 | 98 | 100 | 103 |
| Avg. of All Subjects | A88 B89 | 100 | 104 | 95 | 96 | 108 | 102 | 103 | 92 | A98 B105 | 96 | 104 | 95 | 99 | 100 | 98.7 |

In every case the numbers above refer to average ratios obtained by dividing the average normal performance by the average hypnotic performance (or pseudo-hypnotic with the control subjects) and multiplying by 100%.

Experiment 1 { A = efficiency of performance as judged by amount of errors
B = " " " " " " mean deviations in errors

Experiment 10 { A = score in learning associations
B = score in retaining associations

ing each *S*'s score in each test, did better in hypnosis in 7.5 tasks, equally well in 1.5, and worse in 7.25. This result almost exactly corresponds to the scores obtained by averaging the individual ratios: 7 better, 3 equal, and 7 worse. Their final ratio in Summary I of this paper is 99. If, however, the scores on both parts of Experiment 1 (Accuracy of Movement) be disregarded, the grand average is 101%. Thus the somnambulistic *S*s, the deepest stage of all, are seen to be the most "normal" of all the deeper grades of hypnosis, as far as the results of this research show.

(2) *Results according to tests.* (*a*) *Control Ss.* Table 22, in the original thesis, gives a picture of the way in which the *S*s distributed themselves in regard to the height of their performance in each experiment. Of the 3.2 average control *S*s in all 15 experiments 1.5 did better in pseudo-hypnosis than in the normal state, .18 did the same, and 1.5 did worse. From this angle, then, there appears to be no difference between the "waking" and "pseudo-hypnotic" performance of the control *S*s.

(*b*) *Hypnotic Ss.* For the (average) 12.9 hypnotic *S*s in all 15 experiments, 5.5 did better in hypnosis, .82 did equally well, and 6.5 did worse. From this point of view, then, the hypnotic *S*s on the average did slightly poorer in hypnosis than in the normal condition. It must be remembered, however, that the low scores of all hypnotic *S*s on the first experiment (Movement) make the average hypnotic performance below the average waking performance.

(3) *Results on the basis of physical and mental tests.* (*a*) *Physical tests.* By studying Summary II in this paper, one sees that the light hypnotic *S*s averaged 94%[18]; the deep alert, 100; the deep lethargic, 88; the somnambulistic, 91. The grand

SUMMARY II

| Groups | Physical Tests Exps. 1 - 4 | Mental Tests Exps. 5 - 15 | All Tests Exps. 1 - 15 |
|---|---|---|---|
| I. All Control Subjects | 98 | 106 | 103 |
| II. All Hypnotic Subjects | 93 | 99 | 97 |
| 1. Light Subjects | 94 | 102 | 100 |
| 2. Deep Subjects | 92 | 94 | 94 |
| *a.* Alert type | 100 | 94 | 95 |
| *b.* Lethargic type | 88 | 94 | 93 |
| 3. Somnambulistic | 91 | 103 | 99 |
| III. All Subjects | 95 | 100 | 98.7 |

average of all hypnotic *S*s was 93% (on account of the low scores of Experiment 1); whereas the control *S*s made an average of 98%.

(*b*) *Mental tests.* In these tests the control *S*s have a ratio of 106; whereas all hypnotic *S*s have one of 99. The light hypnotic *S*s show a ratio of 102; the deep alert, 94; the deep lethargic, 94, and the somnambulistic, 103.

[18]See above, p. 219, for an explanation of the percentages given here.

The survey of both types of tests shows that in these experiments in regard to physical tests the control *Ss* did 2% poorer in pseudo-hypnosis, in regard to mental tests 6% better in pseudo-hypnosis; whereas the hypnotic *Ss* did 7% poorer in regard to physical tests, and 1% poorer in regard to mental tests. The grand average shows that while the control *Ss* in pseudo-hypnosis were doing 3% better in all tests, the hypnotic *Ss* in hypnosis were doing exactly 3% worse. From these figures it is plain that it is as easy to explain the behavior of the hypnotic *Ss* in hypnosis as it is to explain the behavior of the control *Ss* in pseudo-hypnosis, in which latter state there was no intimation of hypnosis.

*VI. Conclusions from the Above Experiments.* The experimentation of which this paper is a condensed report has been extensive enough to leave us with some general impressions, which have arisen from a careful consideration of the data. It is clear that some of the popular conceptions in regard to hypnosis which have more or less won their way into scientific psychological thinking are untrue, or need to be restated in a way which casts a different light on the nature of the hypnotic consciousness. Despite individual differences in both the waking and the hypnotic states, differences which are very great, the following facts stand out in the results of the experimenter's work.

(1) The hypnotic state seems to vary as much between individuals and, in the same individual, between different times, as does the normal state. As far as abilities are concerned, there is no unitary hypnotic consciousness which is pretty much alike in various persons.

(2) There seems to be about as much variation in the individual, on the whole, from one normal session to another, or from one hypnotic session to another, as there is from a normal session to an hypnotic session, or from an hypnotic session to a normal session.

(3) An exception to the two statements just made lies in the fact that there is in hypnosis (at least in the somnambulistic stage) greater ability to resist fatigue, to resist pain, and to recall long past events (a later article will give the data on long past memories), on account of the tendency in hypnosis to take a certain mental attitude with conviction.

(4) A possible exception to the first two statements is found in the slightly lower ability in hypnosis to do tasks which require continuous responses, on account of the great relaxation, amounting at times to inertia.

(5) There is in hypnosis a definitely lower ability in the making of muscular movements, on account of the feeling of easiness which influences the judgment, and results in considerably freer,

sometimes almost reckless movements. (Cf. intoxication, which lowers inhibitions and manifests itself in largeness of movement.)

(6) On the whole, there is no noticeable difference between the normal and hypnotic states in the ability of normal persons in the fields of sensation, perception, finer discriminations, present memory (learning and retention), or physical work which does not involve fatigue.

(7) Judging from the great individual variations in both hypnotic and control *Ss*, the past reports of greatly bettered performances of normal *Ss* in hypnosis may be explained by one or all of the following considerations: (*a*) the small number of *Ss*, (*b*) the small number of sessions, or (*c*) the lack of strictly comparable methods in the two states.

(8) Instead of Trömner's twofold division of the various conditions as regards mental abilities (light hypnosis, in which abilities are increased; deep hypnosis, in which abilities are decreased), there seems to be a threefold condition: (1) light hypnosis, in which the performance is fully as good as in waking; (*b*) deep hypnosis (exclusive of somnambulism), in which the performance is perhaps slightly lower than in waking; (*c*) somnambulism, the classical form of hypnotism, in which the normal performance is again equalled.

(9) The so-called hypersensitivity, *e. g.*, to a light touch, seems to be really a fact of subjective sensitivity, or better, perhaps, hyperactivity, *i. e.*, of an attitude of mind which makes the bodily reaction out of all proportion to the size of the stimulus actually felt. Instead of being able to react to a lighter stimulus (as would be the case if there were objective hypersensitivity), the *S* in hypnosis reacts more strongly than in waking to a stimulus which he can, however, feel in waking. That this subjective attitude may continue at the very time that the threshold of sensitivity is raised is probable from the results of Experiment 4. The cause, perhaps, is the removal through dissociation of the ordinary inhibitions.

(10) The possibility remains, of course, that the results shown in this research are a function of the methods of hypnotizing or of the class of *Ss* used. It is thinkable, for example, that results such as S. Alrutz described[19] may be got by using his method or by using his *Ss*; but until the comparative method as described in this paper is assiduously used in such cases, the results of his experiments (*e. g.*, affecting the sensitivity of the hand by passes which never come in contact with the hand) must be considered as without scientific repetition and verification.

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[19] *Proc. Soc. Psy. Research*, 32, 1921, 151.

(11) In order to substantiate differences in mental ability in the two states, the following precautions should be taken: (*a*) there should be a methodology which is almost or quite identical for the two states; and (*b*) the following sets of *S*s should be used: several hypnotic *S*s, preferably of varying depths of hypnosis; several control *S*s who are treated just as the hypnotic *S*s are; and several control *S*s who go through the tests as many times as the two sets just mentioned, but without receiving the suggestion to sleep, or whatever suggestions are given.

The great variability in the performance of normal people without the factor of suggestion, and the astounding effects of suggestion on normal people in waking consciousness, both make the methods described above essential[20].

The hypnotic state is thus seen to be but a modified state of normal consciousness, which is differentiated from other normal states, not so much by way of any changes in physical ability, or of peculiar bodily conditions, as by the tendency to take an attitude suggested (within what limits no one has yet determined) in spite of bodily conditions. Since almost all persons can be hypnotized, and yet not all hypnotized persons (perhaps only one in twenty) can attain to the state of somnambulism, in which the increased abilities referred to in (3) above occur, the differences which appear in somnambulism are differences in the constitution of the persons involved; and would be better described, perhaps, merely as individual differences in normal persons than as differences between the normal and the hypnotic states as such. This would leave the term "hypnosis" with a fairly definite meaning: a state in which a person *will do*, in a *bona fide* manner, possessed of conviction, what he *will not do* in waking life for lack of such conviction.

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[20]C. W. Perky, Experimental Study of Imagination, this JOURNAL, 21, 1910, 422; O. Külpe, Ueber die Objectivirung und Subjectivirung von Sinneseindrücken, *Philos. Studien*, 19, 1902, 508.

# THE PROBLEM OF MEANING IN BEHAVIORISM

By Curt Rosenow, University of Kansas

Many psychologists are strongly attracted to Behaviorism but hesitate to call themselves Behaviorists. In view of this situation, a discussion of the parts of the doctrine which attract us and of the parts which repel us should be worth while; for such a discussion must lead either to a broader definition of Behaviorism or else to a definite break with it on the part of these sympathizers.

It seems to me that all of us who sympathize with Behaviorism at all are attracted by its emphasis on the study of behavior as a biological phenomenon and by its frank recognition of the sterility of the study of states of consciousness. There does not appear to be any one aspect which antagonizes all of us. I find it convenient to distinguish two, as follows. First, at least some of the findings of the introspectionist are accepted as 'states of consciousness' and it is felt to be unscientific to ignore facts, whatever their value; and, secondly, the mechanistic prejudice of the prominent exponents of Behaviorism is felt to be an unnecessary bias which prevents impartial observation and interpretation of the facts of behavior.

In a recent article I presented a Behavioristic point of view which took account of the facts observed by the introspectionist, *but not as states of consciousness*, and which exhibited neither a mechanistic nor a vitalistic bias.[1] In an even more recent article Lashley does accept the findings of introspection as 'states of consciousness' and attempts to show how such facts can be explained by a mechanistic Behaviorism.[2] He attempts to meet the first of the objections stated above and gives the clearest and most systematic example of the second which it has been my good fortune to read. Accordingly I wish to discuss the parts of his doctrine which appear to me most inadequate in order to lead up to further discussion of the position taken by me in the earlier article.

The positions of Lashley to which I object are these. First, I object to the statement that a *complete* description and explanation of behavior can be given in physico-chemical terms[3]. No one doubts that such knowledge as we have of the physics and chemistry of behavior is valuable scientific knowledge, and every scientist hopes to see this knowledge extended. But

[1]Behavior and Conscious Behavior, *Psy. Rev.*, 30, 1923, 192 ff.
[2]K. S. Lashley, The Behavioristic Interpretation of Consciousness, *ibid.*, 30, 1923, 237 ff.
[3]*Op. cit.*, 238.

the dogmatic assertion that the principles of physics and chemistry will some day explain everything that is worth explaining is not only valueless, but, so far as it directs the attention of the investigator away from important facts which do not have the remotest connection with the *known* principles of physics and chemistry, it is vicious. Unfortunately the point is usually discussed under the heading 'Mechanism *versus* Vitalism', as if these two positions were the only ones possible. But there is a third position,—one of indifference to the problem. The typical argument between the vitalist and the mechanist proceeds about as follows. The vitalist challenges: "You cannot explain (let us say) meaning." The mechanist replies with an hypothetical account of the physical events which take place when the phenomenon 'meaning' occurs. The vitalist rejoins: "That does not explain." The mechanist snaps back: "It does"; and there we are. It is obvious that the debaters have divergent views of the nature of explanation. The vitalist demands logical continuity; the mechanist replies with temporal correlation. The problem with which they are dealing can be cast into the following form. If we try to envisage the process of evolution within the categories of space and time, we have no difficulty in granting, in principle, the temporal and logical continuity of physical development. When meaning appears as an observable characteristic of behavior, the temporal continuity of its development from meaningless behavior can be understood, in principle, easily enough. But we are unable to see any logical continuity, and this lack of understanding seems to us more than mere ignorance of detail. At most (if the statement means anything) we can say that prior to the advent of meaning there existed the potentialities which made its evolution possible. But neither the physicist, nor Watson, nor Lashley, nor Bergson has been able to enlighten me about the nature of these potentialities. What follows from this inability to understand? So far as I can see, there follows—nothing, *absolutely nothing*. The sensible thing seems to be to abandon the problem. Meantime I find that I can observe and understand (in a measure) the growth and development of meaningful behavior and, inasmuch as these problems of growth and development interest me, I follow that path.

That leads to my second objection. While I agree that the point of view of psychology should be biological, it does not follow that it should be mechanistic. All branches of biology, including physiology and psychology, have their mechanists, vitalists, and those who are neither. Without attempting to draw a fine line between psychology and physiology, it seems to me that physiology is interested in every aspect of organic behavior, psychology chiefly in its meaning, if there is one. Thus the physics and chemistry of the phototropism are of interest to the physiologist, but the question what, if anything, Euglena means when it reacts to light,—that is, the question whether the phototropism presents a problem which can be called a psychological problem,—that problem is excessively obscure. And it seems to me that one's position in psychology should not depend on the arbitrary solution of an apparently insoluble problem. The case is quite otherwise when we consider human behavior. Here meaning rather than mechanism is of preponderating practical and psychological importance; and if Behaviorism cannot deal with it, so much the worse for Behaviorism.

We come here to the heart of the problem which Behaviorism must face, the problem of meaning. And my third and most serious objection to Lashley's position is that he relegates the description of meaning to Titchener's introspection[4]. Introspection, I shall try to show, is biased observation. The bias is a double one, and becomes particularly unfortunate when applied to meaning. In the first place, introspection finds states of conscious-

[4] *Op. cit.*, 258 ff. "The fact is that meaning, on subjective analysis, reduces to a succession of images accompanied by vague affects."

ness where unbiased observation finds nothing of the kind. Whether a stone is regarded as a physical object or as a state of consciousness does not depend on the technique of observation, but on the bias of the observer. (Meantime the stone, or human behavior, can be described without raising the question.) In the second place, the introspectionist describes his states of consciousness from what Titchener calls the 'existential' point of view. That is, he is interested in what sensation '*is*', not in what it means. And sensation, Titchener finds, is a sensory *quale* of a given duration and intensity. What then *is* meaning? Titchener answers and Lashley quotes approvingly: "An idea means another idea, *is* [emphasis mine] the meaning of that other idea, if it is that idea's context." And again: "Meaning is, originally, kinaesthesis." That is, one sensory *quale* means another sensory *quale*, '*is*' the meaning of that other *quale*,—a kinaesthetic *quale* means a blue *quale*. Surely, neither Titchener's observation nor that of anyone else ever observed so weird a happening. The mere statement is almost a *reductio ad absurdum* of the introspective account of meaning. Now suppose that an individual untutored in introspection is asked to describe the nature of his thoughts. For the sake of simplicity suppose that he depends exclusively on kinaesthesis. After the initial misunderstandings have been cleared away, he may report that he feels either movement or strain of the lips, tongue, eyes, and other parts of the body. If then he is told that these are kinaesthetic sensations, he will have got as far as the average student of psychology ever gets in introspection. If however he perseveres in his observations, he will learn to recognize some of these feelings of strain and movement as *nascent acts* in a way which will clarify the connection of the act with the meaning of the act. For example, the office of one of my colleagues is to the left of mine. A frequent way I have of thinking of him is by going through the bare initial stages of leaving my office to go to his. The eyes tend to move to the left, and there is a slight change in the tonus of many muscles. There is nothing startling or new about such descriptions. What I wish to exphasize is that the *direct observation of a meaningful act* is as genuine and direct an observation as the 'introspection' of a muscular strain. To be sure, if I had not found the historical setting of the act, I might not have been able to *observe* its true character, and it might have remained a meaningless movement to me. But, in the same way, a child who never learns to read will never see anything but black marks on a white background. Learning to note kinaesthetic 'feels' is learning the alphabet of self-observation; learning to observe *meaningful* behavior is learning to read. Only a psychophysical bias coupled with an existential point of view can stop short of a more adequate account of meaning than is furnished by kinaesthetic sensations.

That brings me to my fourth objection, which is that Lashley classes meaning with 'facts of consciousness' and accordingly considers it a *subjective* phenomenon. My position is that meaning is a characteristic of behavior and is *objective*. Of course, the terms 'subjective' and 'objective' are notoriously obscure. Lashley says: "The subjective and objective descriptions are not descriptions from two essentially different points of view, . . . but simply descriptions of the same thing with different degrees of accuracy and detail."[5] Now I have attempted to show above that Titchener's account of meaning is inaccurate and lacks detail not because of the peculiar character of the phenomena observed, but because of the biased point of view of the observer. I have shown that neither the observation of 'external objects' nor self-observation yields facts of an order essentially different from the facts yielded by any possible technique of observation. From such a point of view the subjective *does not exist* except in the mind of the mechanist and the subjectivist. So far as the 'objectivity' of mean-

[5] *Op. cit.*, 338.

ing is concerned, the meaning of an act is often directly observable and at other times it can be inferred on the basis of direct observations. In either case the account is, in principle, verifiable. And observability and verifiability are all the objectivity that science needs. (A word should be said about the verification of an account of meaning. Verification is the presentation of verifiable evidence. Such evidence, aside from the description of the act itself, is the account of the historical facts which led up to it, and correct anticipation of the consequences. An example will be found below. But even though such evidence may convince the most sceptical, it will not constitute *proof* in the sense in which that term is used in exact science. And the reason for that will appear below also.)

We may conclude this part of our discussion by considering Lashley's statement of the problem of psychology, as follows: "To find in the physical world deterministic relations between *nonqualitative* [emphasis mine] discrete entities in time and space which fulfill certain conditions of relationship laid down by subjective evidence."[6] It might be worth pointing out that receptors, neurones, or indeed any entities which are directly observable, have *qualities*. Evidently Lashley has electrons in mind and uses the body only because, to him, it is self-evident that the living organism is a physical thing and its behavior a physical process. Consequently the meaning of that behavior becomes 'subjective'. The reader's attention is directed chiefly to the logical connection between the physico-chemical bias and the subjectivity of meaning. The physico-chemical bias itself is not worth opposing, because no living psychologists are actually at work at physico-chemical problems nor will they be in the near future. The actual explanations of the mechanistic Behaviorist are being given in physiological terms. And what I object to is not the physiological terminology, but the *falsification of the observable so-called subjective facts.*

The chief problem of psychology, as I see it, is to give an adequate, correct, and verifiable account of the meaning of behavior. The total problem of physiology and psychology has been stated adequately by Watson as that of prediction and control of behavior. But he who understands the meaning of behavior so far as seems possible will be able to predict and control human conduct in the vital problems in which Lashley professes interest far more adequately than he who knows all the physics, chemistry, and mechanistic physiology which we shall know for some time to come. At any rate, that is the path which has always been taken by those who seek to understand and influence men and women in a practical way, and it is, in effect, the path taken by the psychiatrist and the psychoanalyst. Psychology as the science of a bodiless mind joined to a mindless body has contributed almost nothing, and a Behaviorism which attempts to explain meaning in terms of meaningless movements will not contribute more. Before Behaviorism can attack the genuine problems of the meaning of behavior it must clear out of its path the metaphysical problem which it has created for itself by insisting on the subjective (or epiphenomenal) character of meaning. The rest of this paper will be devoted to the discussion of

[6] *Op. cit.*, 329.

behavior and meaning as *facts*, without asking or caring whether the facts are physical or mental, subjective or objective.

In a previous paper I put forward the claim that meaningful behavior is a directly observable objective fact and that it should be the most fundamental and basic category of psychology. I supported that claim by description and discussion of various types of meaningful behavior by means of which I attempted to make this idea intelligible and plausible.[7] Since that time various critical suggestions have reached me, three of which I mean to utilize in order to develop this idea further. (1) It was said that my treatment implies that the behavior of even the lowest forms of living organisms is meaningful or conscious behavior; (2) it was pointed out that the absence of a definition of meaningful behavior weakened the article; (3) the statement was made that these theoretical views would mean more if it were shown what relation, if any, they have to the experimental work which is being done by psychologists. In what follows I shall assume that the reader is familiar with the former article.

Taking up the first point, I start with the behavior of the *human* animal. It seems to me an indisputable fact that the meaning of human behavior is, at times, directly observable. The objection that the term 'meaning' is anthropomorphic seems ridiculous, so long as we confine ourselves to the description of human behavior. How else is human behavior to be described, if not in terms which have evolved for that very purpose? When we come to the higher vertebrates it seems equally obvious that they see, hear, smell, fight, and engage in other forms of meaningful behavior analogous to human behavior. On the other hand it should be equally obvious that there are notable differences between human and infrahuman behavior. But that is a question of detail which, with one exception, I did not discuss.[8] And, so far as I can see, one is not under logical obligation to discuss such detail. But I deny categorically that anything whatever can be deduced regarding the behavior of protozoa from the established fact that some human behavior is meaningful, except perhaps that the earliest forms of life were of such character as to make possible the subsequent evolution which as a matter of fact did take place. Such a statement is entirely different from the statement that the behavior of protozoa is meaningful. And I deny that a solution of this problem is necessary to an adequate understanding of human conduct.

Of course the issue ultimately involved at this point is a metaphysical one; and in 'Behavior and Conscious Behavior' I protested against the introduction of *any* metaphysical assumption into psychology. Weiss holds

[7] Behavior and Conscious Behavior, *Psy. Rev.*, 30, 1923, 192 ff.

[8] The exception is that my discussion implied that only animals with well developed language-habits are capable of self-conscious behavior.

that that amounts to a refusal to discuss fundamental issues;[9] and, at the expense of a brief digression, I wish to discuss his point. So far as the refusal to discuss fundamentals is concerned, I had supposed that in discussing consciousness and behavior I *was* discussing fundamental ideas. But I do deny that an arbitrary solution of a metaphysical problem yields a sound foundation for a psychology which deals with the empirical facts of human behavior. The metaphysical foundation which Weiss has used may serve as another illustration.

Weiss holds "that all human conduct and achievement reduces to *nothing but*: (*a*) different kinds of electron-proton groupings characterized according to geometrical structure; (*b*) the motions that occur when one structural or dynamic form changes into another." Accordingly the individual is an electron-proton aggregate and a function (in the mathematical sense) of the changes that are occurring in all other electron-proton aggregates.[10] That is the foundation. Then Weiss proceeds to classify the acts of individuals as (1) biophysical, *i. e.*, anatomically and physiologically similar acts, and (2) biosocial acts, which are socially equivalent because they release socially similar responses in others. Thus various ways of accepting an invitation are socially equivalent if they would be responded to in practically the same fashion by the one who extended the invitation. That is the psychological superstructure built upon the metaphysical foundation. Now the superstructure is not deducible from the foundation nor does it have any logical connection of any kind with that foundation. No arguments either for or against the accuracy, utility, or significance of Weiss' dual classification can be drawn from his electro-proton hypothesis. The classification, separated from its metaphysics, could be accepted without modification regardless of one's theory of reality. In fine, the metaphysical foundation of Weiss, far from being fundamental, is irrelevant to the rest of his psychology.

I come now to the fact that my former article lacked a formal definition of meaningful behavior. Every science, so far as its structure is logical and systematic, has at its foundations a set of *undefined terms* and *unproved assumptions* about these terms. Inasmuch as I believe that the term 'meaningful behavior' should be one of the fundamentals of psychology, I cannot define it by reducing it to simpler terms. To do so would be tantamount to the admission that these simpler terms are themselves the real fundamentals. Nevertheless it would be unwarranted to throw the term at the reader without an attempt to make clear to him the nature of the concrete phenomena which it denotes to me. Logically I should be perfectly justified in using an undefined term and making assumptions about that term, but psychologically such procedure would leave the term abstract, empty, and without relation to concrete experience. Consequently I have tried to point at, describe, and discuss the concrete phenomena which the term denotes to me. For example, in 'Behavior and Conscious Behavior,' I have discussed the behavior of seeing as one of the phenomena which the term denotes. Also I have discussed the alleged fact (logically, the unproved

[9] A. P. Weiss, Behavior and Behaviorism, *Psy. Rev.*, 31, 1924, 35.
[10] *Op. cit.*, 39.

assumption) that the acting organism is not always aware of the meaning of its meaningful acts. In the rest of this article I shall offer additional description and discussion. In lieu of a formal definition, which, as I have shown, is logically impossible, I shall make a brief statement intended to sum up the discussion of the former article as well as the discussion which is to follow in this article. No other 'definition' of a fundamental idea is logically possible.

When a psychologist studies a human being for the purpose of understanding him and his behavior, he arrives after a time at what he believes to be a true statement of the environment of this individual or as near to such a statement as is possible. At the same time he recognizes (or should recognize) that the environment as experienced by the observed individual is more or less different from the environment experienced by the observer. In order to understand the individual the observer endeavors (or should endeavor) to make clear to himself the nature of the environment *as experienced by the individual.* As a matter of definition, I shall call the environment as experienced by the observer the true environment, or briefly the environment, and I shall call the environment as experienced by the observed individual the experienced situation, or briefly the situation, and the two terms will be used in this sense throughout this article. I am now ready to make a brief statement to take the place of a formal definition of the term 'meaningful behavior'. By means of meaningful behavior the organism enters into relations with its environment.[11] Each relation is at one and the same time the behavior of experiencing a situation and the behavior of acting with reference to the experienced situation. *By definition,* the behavior of acting with reference to a situation and the behavior of experiencing that situation is a single phenomenon; the behavior of acting with reference to a situation *is* the behavior of experiencing that situation. A concrete illustration would be an individual who establishes a relation with an environment consisting in part of a white sheet by seeing a ghost. By means of 'seeing-the-ghost' the organism enters into a relation with its environment; seeing-the-ghost is the behavior of experiencing the ghost; seeing-the-ghost not only leads to further behavior with reference to the ghost, but is itself behavior with reference to the ghost.

Let us now discuss this definition. So far as the environment is concerned, the psychologist should describe it as he sees it *after mature deliberation.* If he views it as an electron-proton aggregate he should describe it in that way (without much detail,

[11]These relations which the organism enters into are by no means the only relations of the organism to the environment which can be observed.

I fear). But I insist that he should not let his view of the environment falsify his description of the situation. If I saw a ghost, it does not describe *the situation* to say that I saw a white sheet hanging from a clothes-line.[12]

Again, I have asserted that the meaningful act is at one and the same time a mode of experiencing and a mode of acting. In other words, I assert that the distinction between awareness and action is valid logically, but invalid phenomenologically. The distinction has arisen because in the practical exigencies of life we usually describe what we have experienced, not our modes or experiencing,—we discuss what we have seen, not the behavior of seeing. The distinction is valid and useful, just as the distinction between the pitch and the intensity of a sound is valid and useful even though the two are inseparable aspects of a single phenomenon. But, considering the meaningful act as a concrete phenomenon, awareness and activity are inseparable. The distinction rests upon an analysis of language and the fallacy arises because terms such as seeing, fearing, and shrinking, which should denote partial aspects of single unitary activities, are taken to denote separate activities. Seeing is (by definition) the immediate automatic response by means of which the organism establishes relations with the environment *via* the retina, not *with* or *to* the retina. And *all of this response*, the visceral part as well as the part played by the striped muscles and *by the eye*, is the act of seeing. For, *by definition*, the organism does *not* respond to a stimulation of the sense-organ, but the sense-organ is a part of the organism which reacts to the environment. Terms such as seeing, hearing, etc., as used ordinarily, denote the part played in such reactions by the sense-organ and connote the sensory *quale* of the experienced situation; terms such as shrinking, cowering, running, striking, etc., denote primarily the part played by the skeletal muscles without specific connotation regarding the qualitative character of the situation with reference to which the act takes place; and the emotional terms of non-technical language are more or less vague both as to the sensory *quale* of the situation and as to the part played by the effectors in the response. But shrinking from danger is an act which takes place *via* some sense-organ, not to a sense-organ, and the emotional *quale* is as truly perceived as part of the experienced situa-

[12]We may note in passing that if I see a ghost I do not necessarily observe myself seeing the ghost, but the observer *necessarily* does so. We need not therefore debate the question whether the self is a 'content of consciousness'. In observing the conduct of an organism one necessarily observes the organism, and that is true also when the organism observes its own conduct. Accordingly the classical problem of the self as content reduces to the problem of the meaning of the word 'I' (or some equivalent) in various situations.

tion as is the sensory *quale*. In certain instances, and for practical purposes, it is legitimate enough to speak of first seeing danger and then shrinking from it; but a careful psychological statement should state that the activity of experiencing a situation to be shrunk from by means of the behavior of shrinking from it is preceded by the activity of experiencing a somewhat different situation, *e. g.*, a situation to be investigated anxiously, by means of the behavior of investigating it anxiously. As the behavior changes, so the situation changes. Ordinary language, legitimately enough, denotes the earlier situation by the general term danger, which means a situation which can be responded to in a variety of ways without designating the specific ways which actually are occurring.

I wish to emphasize once more that it is the behavior of *experiencing-a-situation* which constitutes the relation of the organism to its environment. The situation is what it is only by virtue of being experienced in that way. Consequently any attempt to describe this relation must include not only an account of the behavior of experiencing but also an account of what is experienced. Seeing-a-ghost is a single unitary phenomenon. The seeing cannot be abstracted from the ghost except as length is abstracted from width. Neither has phenomenological existence except as a characteristic of a phenomenon.

Before attempting to discuss more at length the way in which meaningful behavior means its stimulus or situation, I wish to review certain other general characteristics of meaningful behavior. In ordinary usage the word meaning is used to indicate (1) a characteristic of experience, and (2) a characteristic of behavior. Frequently the two usages are interchangeable. Thus I may speak of perceiving a threatening cloud or of meaning to seek shelter. But in either case my relation to the environment is my meaningful behavior toward it; and when the state of affairs is envisaged in that way the problem of the locus of meaning is solved. It is the relation of the organism to its environment which means. Consequently I have argued that the psychologist should deal with meaning only as a characteristic of behavior; and that is the usage adopted in this paper.[13]

Again, the idea of meaningful behavior has something in common with an idea in good repute with the physicist, viz., the idea of motion. No matter how far we push the process of abstraction, we cannot abstract the ideas of time and direction away from either. If we do, there is left neither motion nor behavior. The motion of a body or the behavior of an organism is not a thing which '*is*'; it is an event which happens. It is in its very essence historical and cannot be understood without refer-

[13] Cf. Behavior and Conscious Behavior, 211 ff.

ence to its history. There are times when the relevant history of a moving body can be perceived in a fraction of a second sufficiently well to determine the direction of the movement with an accuracy which suffices for practical purposes. The same thing is true of the meaning of acts. At other times extended observation and a process of inference may be necessary to determine the direction of the one or the meaning of the other. Beyond this point, however, the analogy ceases to hold; for the direction of a movement can be computed with absolute accuracy (in theory), whereas it is frequently impossible, even in theory, to state the meaning of an act with as much rational clearness as might be desirable. This difficulty will be discussed further presently.

Again, in 'Behavior and Conscious Behavior' I cited G. H. Mead to the effect that the gesture is an unfinished social act which means the completed act. Thus the beginning of an attack is *perceived* as the beginning of an attack, not as a movement the meaning of which must be inferred. I wish to make two additions. In the first place, the beginning of an attack is what it is whether or not it has been correctly perceived at the time of its occurrence. In the second place, an unfinished act need not be a *social* act in order to mean whatever it means. In any case, the meaning of an unfinished act can be stated adequately only with reference to the total act of which it is the beginning. Accordingly Mead's formula is applicable to all kinds of acts, social and non-social.[14]

In order to illustrate and elaborate these points, let us examine first a simple form of behavior and then a complicated case. The picking up of a pencil will do for the simple form. Usually that will be part of a larger activity, such as the jotting down of a memorandum which in turn is noted for some purpose or other; but let us neglect that. An account of the meaning of this act is given by the common-sense description, viz., picking up a pencil. But it is worth noting that that meaning is not a static fact or relation; that it progresses as the act progresses; and when the activity comes to an end, the meaning ceases, just as motion ceases when a moving body comes to rest. A completed act does not mean,—it has meant. If, however, the act is interrupted before completion, the incomplete act can be described adequately only with reference to what the act would have been, if it had not been interrupted, for up to the moment of interruption the act meant just that.[15]

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[14]We must be careful however to distinguish the consequences of the act from the meaning of the act. For example, the sitting hen does *not* mean to hatch chicks, and the human animal not infrequently does *not* mean to become a parent. The distinction can be made on the basis of relevant evidence and it is the task of the psychologist to find such evidence.

[15]In order to ward off a misunderstanding which occurred in the case of a critic who saw this paper in advance of publication, let me say that the discovery of what a completed act has meant may be as important psychologically as finding what an incomplete act has meant or means while it is occurring. Thus an homicide might be described as a completed act by a

Our illustration of a simple meaningful act has been reasonably clear, if we do not deprive ourselves of common sense in the interest of a mechanistic bias. It must be borne in mind, however, that a part of this clarity was achieved by abstracting a trivial act from its larger setting. A concrete act is never merely the picking up of a pencil; and if we had taken a larger setting into account, a precise and exhaustive account of meaning would not have been possible. Let us therefore examine a complex case. Some years ago I was consulted by a patient who suffered from attacks of tachycardia for which no organic cause could be found. He had been told by physicians that his case was 'mental'. Assuming the medical findings to be correct, the rapid heart-action could be nothing but one of the phenomena of strong emotion, but that told me neither the nature of the emotion nor its occasion. A study of the life-history obtained from the patient himself revealed a history of partial failures in business and of misadventures in love. At the time he came to me he was engaged in an important undertaking the outcome of which seemed doubtful to him. I concluded that he had lost confidence in himself, was afraid that he was going to fail in the undertaking in which he was engaged, and feared that he would make a failure of life. At certain times this fear degenerated into panic, and that, I concluded, was the tachycardia. Of course, the patient did not admit all this to himself. As a matter of fact, the immediate problem which confronted him was not difficult, and there was no reason why he should not look forward to a successful issue with confidence. Without attempting to explain the tachycardia to him, I succeeded in instilling a little confidence into him (perhaps only temporarily) and the tachycardia disappeared. That is a common-sense account of the case. I shall attempt now to deal more adequately with the concrete meaningful behavior as it actually occurred. The behavior in question is, of course, not merely the tachycardia but the total behavior of which that was a part.

I have said that the meaning of an incomplete act can be described only by describing the total act of which it is the beginning. If we regard the rapid heart-action as the beginning of an activity, our problem is to describe the rest of the act, *i. e.*, what would have happened if the act had been completed. In the first place and in part, the tachycardia was the beginning of an attempt to escape, by means of *running*, in the literal 'physical' sense, from a fearful situation.[15] That act, of course, was irrational; but emotional reactions frequently are. Our subsequent analysis will not and should not make the meaning of the actual act more rational, but it should make more intelligible the fact that in spite of its vagueness and irrationality the act had a *more or less* definite meaning. The tachycardia, then, was not only an initial stage of running away, but it was also an integral part of the act of *perceiving* a *fearful* situation. In 'Behavior and Conscious Behavior' I tried to show that perceiving is a form of behavior, that we *perceive* not only actualities, but possibilities and probabilities, and that such perceiving *is* the occurrence of more or less automatic and habitual forms of meaningful behavior. These principles will make the tachycardia more intelligible. In perceiving we perceive not only objects, but total situations. Conventional psychology has made a sharp distinction between cognitive and affective states of consciousness and has, arbitrarily, abstracted all affective factors from the 'percept'. But if we attempt to study perceiving as meaningful

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mechanistic Behaviorist without taking account of the meaning of the act, but from the point of view of the law and of common sense such a description would be highly inadequate. If, for example, the individual who committed the homicide meant to preserve his life, the meaning of this completed act would be highly relevant.

[15]As a matter of fact these attacks usually came on after dark, and the patient would rush out for long walks.

behavior we cannot afford to follow this line of cleavage. If, as I have suggested, we perceive with our muscles and glands as well as with our sense-organs, then the heart and the viscera can enter into the behavior of perceiving as well as the striped muscles. If perceiving is in any sense a total reaction of the organism, then the fact that there has not been an important change in visceral activity does not mean that these visceral activities are not a part of perceiving, but that the behavior of perceiving has not been modified so far as visceral activities are concerned. An illustration would be the calm perceiving of a dangerous situation as contrasted with the perceiving of a fearful situation. In the second case we do not have the cognition of danger *plus* the emotion of fear, but we have the perceiving of a terrifying situation. The visceral behavior is not the response to a fearful situation, but a situation is perceived as fearful because part of the response is a change in visceral activity. Of course, the self-conscious human adult can, and sometimes does, give his attention to his own behavior of fearing; but no doubt all of my readers will recall situations which *looked, sounded,* or *felt* tense or ominous or ridiculous or the like in a way which will make them realize that such descriptions are not mere figures of speech, but accurate descriptions of concrete experiences. In such situations possibilities and probabilities are *perceived*, not thought of. To be sure, the perception is vague, and when we attempt a description of these perceived possibilities and probabilities in verbal terms we find the task extremely difficult. Nevertheless there occurs in such situations more than 'a pointing from nothing to nothing',[17] and it is the task of the psychologist to give a rational account of such irrational behavior. As I have said and as we shall see, that can be accomplished by viewing the behavior as an historical event with a past and a future.

In consequence, then, of certain developments in his affairs our tachycardia patient *perceived* a terrifying situation. Let us suppose that he became acquainted with these developments through reading a letter. The response was in part visceral and persisted after the letter had been read and he had stopped thinking about it. As I have suggested, we have at all times an appreciation of the emotional aspects of our situation. Thus the individual who is grieving tastes bitter, tasteless, or revolting food, breathes an oppressive atmosphere, thinks depressive thoughts, and the like. With it all there is a constant (usually unobserved) tendency to escape from the sad situation. In the same way our patient continued to experience a fearful situation long after he had stopped thinking about it. Occasionally the visceral part of the activity became intensified into tachycardia.

For the physiological details of the visceral reaction we may accept the 'rather probable guesses' of the typical Behaviorist; for its meaning we shall consult its history. On a number of previous occasions in his life my patient had experienced 'unfavorable developments'. In each case the reaction had been partly visceral. Probably, at first, the visceral reaction did not persist. But on each occasion his fears had proved to be justified; failure had ensued. So on subsequent occasions his visceral reactions became more persistent, and more definitely meaningful. And they *meant* what his past experience had taught, viz., failure. The details of the failure were foreseen only so far as they were foreseen, *i. e.*, so far as he brought them to his own attention by means of overt or nascent gestures. Apparently very little of such foreseeing took place. The behavior was largely non-social and non-gestural. The probability of failure was not recognized as such; it was not thought of in that way; it was not *known*. The best possible way of describing the phenomenon is to say that the individual *feared* failure. If the patient had said "I fear failure," no one would deny that his

[17]Cf. K. S. Lashley, The Behavioristic Interpretation of Consciousness, *Psy. Rev.*, 30, 1923, 258.

verbal behavior meant what it did mean. *All I claim is that the statement would have been true, if he had made it.* And the fact that he failed to perceive the meaning of his own behavior should not prevent our doing so. But neither the acting individual nor any one else can ever know in precise, accurate detail what an emotional reaction means, *because it does not mean accurate and precise detail.* It is not that kind of behavior. It is, essentially, a vague *perceptual* summing up of past experience.[18]

The description of such situations and of such meaningful behavior cannot be given, at the present time, in the technical terms of any of the sciences. Certainly neither the sensations and affects of conventional psychology nor the vocabulary which Behaviorism has borrowed from physiology will serve. At the present time the only language which is adequate is the non-technical language of highly cultured nations, presumably because this vocabulary has been developed by individuals who, unlike the psychologist, have faced the real problems of human life. A usable terminology has been developed by Freud,—usable, because many of his terms sum up lengthy description of observable characteristics of human behavior into single terms. But the Freudian vocabulary is objectionable because it substitutes false rationalizations for vague meanings. Take for example the 'Oedipus complex'. Freud is right when he claims that marital unhappiness comes back in certain typical cases to the love of the wife for her father. But as a rule he is mistaken when he claims that this love is an unconscious desire for sexual union with the father. A young woman who has been petted, protected, humored, and treated as a lovable but irresponsible child by her father up to the day of her marriage, and who has enjoyed being treated in this fashion, is not likely to be happy with a husband who treats her as a responsible adult. Such a woman may love her father and be unhappy with her husband. But the important factor is a (more or less) vague dissatisfaction with the present situation and a (more or less) vague longing for a return to the earlier satisfying situation. There is no unconscious desire. The behavior means—is—the beginning of the act of leaving the husband to return to the father.

Suppose now that we were able to make a complete physiological analysis of the complex forms of behavior which we have been discussing. Very likely such an analysis would throw additional light on the cases, but for such light we must await the progress of physiology. Certainly the 'rather probable guesses' which Lashley feels he can make are not serviceable. For when Lashley says that "he is hungry and purposes to have beefsteak and onions for dinner" these words are not, as he claims, merely accepted names for the fact that certain physiological changes are occurring.[19] In that sense they are accepted only by a small (and mistaken) group of Behaviorists. By the rest of us they are accepted 'names' for what he means to do. That is, they give us the meaning of the behavior which he attempts to describe in physiological terms. But if the psychologist knows the meaning of behavior, he can dispense with rather probable guesses about its physiology. I do not mean to say that physiological observations cannot give us valuable hints about the meaning of behavior. Recent work on the physiology of deception illustrates how physiology can help psychology. I do affirm that Lashley is putting the cart before the horse when he says that the aim of

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[18]Half the difficulties of psychology result from our unsuccessful attempts to force the description of vague meanings into the verbal categories evolved by logicians and the disciples of exact science. But what is vague '*is*' vague. And so long as science remains the attempt to find out what actually occurs, so long will science reject the false simplifications of the mechanist and the equally false rationalizations of the teleologist.

[19]*Op. cit.*, 350.

self-observation should be the discovery of cues to physiological problems.[20] Physiological technique is of service to psychology because it enables us to observe details of behavior ordinarily hidden from view, not because it gives us the *objective* correlates of *subjective* facts.

I come now to my last topic,—the bearing which the views expressed in 'Behavior and Conscious Behavior' and in the present paper have on experimental technique. First let me ask: "What has experimental psychology contributed to the interpretation of such cases as our tachycardia case?" So far as I can see, one does not need to be a psychologist or Behaviorist in order to understand that repeated failure is likely to discourage the individual more or less permanently; that failure in important undertakings is more likely to have this effect than failure in trivial affairs; and that business and love are two of the most important affairs in which men engage. With a history of such failures before one, one *should* look for signs of discouragement and fear. Nevertheless the ordinary cultured individual is not likely to arrive at a correct interpretation of such a case. The ordinary individual does not make it his special business to interpret human behavior except so far as seems necessary to carry on his own affairs. He takes account of the point of view of the other individual only in order to effect a sale, make a good impression, or even in order to win friendship or love. But, in the first place, many of us do not attempt to understand others even where it would be plainly to our advantage to do so. And, in the second place, even when we do so we seek a means to an end, not an end in itself. Accordingly an utterly inadequate amount of time and energy is devoted to the task of attempting to understand others. Obviously, the man whose chief interest is to reach an understanding of human conduct, who brings to bear all his knowledge and experience upon this single task, and who pursues this end even when he is not confronted by an immediate problem, will have an advantage over the ordinary cultured individual whose main interests lie elsewhere. On the other hand, it is a question which can be raised in all fairness whether the laboratory psychologist is really interested in human conduct in this vital way. Most of us have come to see that analysis of states of consciousness into sensations and affects does not lead in that direction. I have attempted to show that analysis of human behavior into meaningless physiological components is predestined to be equally futile. And whatever connection with sterile theory the fact may have, *it is a fact* that the human behavior studied by our experimentalists is trivial behavior studied under artificial conditions. The individual, as such, does not

[20] "Its (introspection's) avowed aim must be the discovery of cues to physiological problems"; *op. cit.*, 352.

concern the experimentalist. He experiments with 'subjects,' not with individuals. Individual histories and individual problems are unknown and do not enter into laboratory problems. Frankly, I cannot see that the laboratory work that has been done since the beginning of experimental psychology has added materially to our understanding of human conduct, nor has there resulted from it any considerable advance in psychological theory. We have advanced, but the advance has been due to more accurate and less biased self-observation, to the advance of biology, to the spread of knowledge about and the increase of thoughtful consideration of our major social problems, to better and more accurate knowledge of the behavior and conditions of other times and other races, and to the observations of workers in fields on the outskirts of academic psychology, such as 'mental' disease, crime, and the like.[21] If our experimentalists would care less about making psychology an 'objective science' and more about contributing to the *facts* from which progress has come and will come, they would join the other workers who observe the human animal in its natural habitat. The important behavior-patterns of human beings are social through and through. Any considerable increase in our knowledge of the formation and meaning of these patterns can come only through the study of the actual formation and functioning of these patterns and of the actual conditions under which they are formed. The home, the school, the church, court, factory, office, shop, etc., are the workshop of the psychologist. For science is not playing with apparatus in the laboratory, but the search after truth.

In conclusion and by way of brief summary, I shall enumerate what seem to me to be the essential positions of this paper, as follows.

(1) I have tried to show that a mechanistic Behaviorism cannot deal with meaning because meaning is not a mechanistic category. Lashley in particular attempts to meet this difficulty by adopting a quasi-dualistic position, and solves his problem by finding the physiological correlates of the sensations and images into which meaning is resolved by introspection. But technical introspection (from the 'existential point of view') attempts to resolve the dynamic fact of meaning into static components and accordingly must fail.

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[21] Psychology has made a genuine contribution to the understanding of the human individual in the mental test, but this advance has come as an extension and adaptation of the old-fashioned pedagogical examination, not as a development of the laboratory experiment. But lack of space and time forbids an adequate discussion of this topic.

(2) I have held that meaning is the differentia which distinguishes human behavior and the behavior of some animals from the behavior of inanimate objects, and that meaningful behavior should be one of the fundamental categories of psychology.

(3) I have shown that a fundamental category cannot be defined by analysis into simpler categories, and that it is possible to define such a category only by means of description. Such a description of the meaningful act has been given in as general terms as possible.

(4) I hold that the meaning of an act is at times evident to direct observation and is accordingly an objective fact. At other times the meaning of an act can be inferred from the history of the act.

(5) I hold that the meaning of a concrete, specific, human act is *essentially* more or less vague, but that nevertheless it is frequently describable with sufficient accuracy for the purpose of a truthful and useful description. In order to make the last two points, a concrete illustration was considered necessary.

(6) I have replied to the criticism that the views expressed in this and in a former paper have no bearing on the work of the experimentalist by alleging that the present-day experimentalist does not deal with the important concrete facts of human life. The clinician who does deal with such facts will, I trust, find these ideas useful.

# THE "GLASSY SENSATION"[1]

By E. F. Möller

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## *Introduction*

In 1920 Schumann[2] described a hitherto unrecognized sensation, which he called the 'glassy sensation'. The name arose from the reports of *O*s who, under certain circumstances, described empty space as filled with a "glass body, in which there are very small particles,"[3] with "frozen air,"[4] or "transparent ice."[4] It was the object of the present investigation to secure an exact description of this 'sensation' and to determine the special conditions of its arousal. We have failed entirely in the latter half of our task; but, on the other hand, we have secured a large amount of material which we think points to the cooperation of the centre, if not to a central origin. We believe, that is, that the 'glassy sensation' is essentially imaginal, of the nature of a tied image, and not sensory.

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[1]From the Psychological Laboratory of Cornell University.

[2]F. Schumann, Die Repräsentation des leeren Raumes im Bewusstsein *Z. f. Psychol.*, 85, 1920, 224-245.

[3]*Op. cit.*, 230.

[4]*Op. cit.*, 232.

The history of the problem goes back to Hering (1879). "During the day, one sees what is called empty space between oneself and the object which one sees quite differently from at night. The increasing darkness lies not merely on the object, but also between oneself and the object, until finally it veils the object completely and fills the space alone."[5] Karpinska, working with the stereoscope, found that the object which her *Os* saw, though given objectively only by black boundary lines, was made up of an 'etwas'. "This material is something quite airy; transparent. The *Os* designate it as transparent paper, gelatine, glass. a very thin indefinable material; thickened or crystallized air. It is air and yet not air."[6] The transparent *Zwischenmedium* of Jaensch, which may be either colored or colorless, belongs as a space-filling medium to the same category.[7] It remained, however, for Schumann to undertake a special investigation of this visible intermediary, with the result that he raises it to the level of a 'sensation'.

Schumann, using the Zeiss stereoscope, presented to his *Os* some of the Martius-Matzdorff stereograms, which showed figures outlined in black upon a white ground, and some stereoscopic pictures of streets. He asked whether there was anything 'visible' between the lines of the figures or the houses on the streets. The reports showed that "the surfaces of the figures appear to be made of a glassy material . . . The content is a less compact medium, but it is like glass and has many points localized at different distances in it." A stereogram of a marketplace appeared "as if the whole was enclosed in something which was 'like quite clear ice'," and in which, too, there were small points, "like the finest dust."[8] The *Os*, upon questioning, characterized this filling as "clear mountain water," "quite transparent ice," "frozen air," "a colorless filling," which is "more or less compact," "condensed," or "thin."[9] They stated definitely that the glass did not belong to the light or to the color series, that it was a new quality different from those of the recognized visual sensations. When, repeating Jaensch's experiments, they observed the *Zwischenmedium*, they declared it to be qualitatively the same as the stereoscopic filling.[10] They also found the 'glass' with pairs of identical figures viewed in the stereoscope; and, of their own accord, reported instances in which they had observed it in nature.[11]

The conditions for the arousal of this 'sensation', Schumann finds, are (1) plasticity, which is aided by eye-movement; and (2) the localization of

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[5]E. Hering, Der Raumsinn und die Bewegungen des Auges, in Hermann's *Handb. d. Physiol.*, 3, 1879, 573.

[6]L. v. Karpinska, Experimentelle Beiträge zur Analyse der Tiefenwahrnehmung, *Z. f. Psychol.*, 57, 1910, 44f.

[7]E. Jaensch, Ueber die Wahrnehmung des Raumes, *ibid.*, Erg. Bd. 6, 1911, Kap. 6, 244-307. Cf also: W. Fuchs, Experimentelle Untersuchungen über das simultane Hintereinandersehen auf derselben Sehrichtung, *ibid.*, 91, 1923, 166; H. Hofmann, Untersuchungen über den Empfindungsbegriff, *Arch. f. d. ges. Psychol.*, 26, 1913, 112ff.; E. Rubin, Visuell wahrgenommene Figuren, 1, 1921, 61-66; G. E. Müller, Zur Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufes, *Z. f. Psychol.*, Erg. Bd. 9, 1917, 408f.; D. Katz, Die Erscheinungsweisen der Farben, *ibid.*, Erg. Bd. 7, 1911, 17f., 309ff; K. Bühler, Die Erscheinungsweisen der Farben (*Handbuch d. Psych.*, I., i.), 1922, Abschn. I., esp. 21 ff., 63 f.

[8]F. Schumann, *op. cit.*, 230.

[9]*Op. cit.*, 232.

[10]*Op. cit.*, 233.

[11]*Op. cit.*, 237.

small points in different planes. The latter condition may have its value in that it brings about roving attention, or roving attention accompanied by eye-movement, or varying degrees of compactness of the filling.[12]

This 'glassy sensation', which Schumann proposes as the sensory representative of empty space, has already found an antagonist in von Frey, who declares that the phenomenon which Schumann observed is due merely to a technical imperfection in the paper of the stereoscopic slide, and that aside from the effect of such imperfection there is nothing present in the experience.[13] Von Frey repeated Schumann's experiment with the stereoscope, using for his slides both stereoscopic and identical photographs of simple objects, printed upon dull and upon glazed paper. He remarked an increase in the size of the object which he saw in the stereoscope; an increase in existing lustre, or an addition of lustre to dull surfaces; and finally, the presence of a fine dust which filled the space in which the object stood. The dust appeared the more clearly, the duller the paper of the slide; and it arises, according to von Frey, simply because "the maximum and the minimum points of brightness arising from the grain of the paper fall on disparate, not on identical, points of the two retinas, and according to the degree of disparity are seen in different depths. The impression is favored by skillful lighting, if the roughness of the paper thus comes out clearly."[14] This dust-filled space von Frey identifies with the glass of Schumann. He also finds it in nature, when the air is filled with smoke, fog, or dust. That the evidence points to a new 'sensation' he denies, (1) because the fog-product of stereoscopic vision is associated with a definite technical imperfection of the picture; (2) because differences of intensity, which should enable one to judge the depth of the space perceived through the sensation in question, do not exist; (3) because, when the usual criteria for the estimation of distance are not present, as, for instance, in the clear air of high mountains, serious errors are made in the estimation of distance; and (4) because "truly transparent bodies are not perceived except through polish or blemishes on the surface or inside the body, or on the ground of known optical criteria."[15]

Both Schumann and von Frey find suspended in the space within and about their experimental objects certain small flecks or dust-like particles. But Schumann declares that "if these points are not in a plane, but are arranged at different depths, they appear to lie in a glass body . . . If they are all localized in the same plane, the arousal of a plane (glass) surface is aided . . . Sometimes there appears before the whole image a glass plate with little flecks upon it."[16] Schumann thus observes, in addition to the flecks, a glassy medium, in or upon which these bits of dust are contained. Between the flecks there is the glass of the 'glass body'. Von Frey, on the other hand, admits only the flecks, between which he sees nothing. "The object . . . stands in a space which is filled with fine dust . . . This dust acts as an air perspective . . . The 'air or glass impression' of which Schumann speaks is only another name for what I have called 'dust and lustre'."[17]

Schumann and von Frey are agreed as to the flecks; they disagree whether there is anything visible between or around the flecks. On this main point the results of our observations sustain Schumann.

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[12] *Op. cit.*, 238.

[13] M. v. Frey, Über die sog. Empfindung des leeren Raumes, *Z. f. Biol.*, 73, 1921, 262-266.

[14] *Op. cit.*, 264.

[15] *Op. cit.*, 266.

[16] F. Schumann, *op. cit.*, 239.

[17] M. v. Frey, *op. cit.*, 264.

Our experiments naturally group themselves under four heads: (*A*) experiments with the stereoscope; (*B*) experiments with skeleton forms; (*C*) experiments with mirrored forms; (*D*) miscellaneous experiments. A detailed account of method and apparatus appears under every head. These groups do not represent the chronological order of the investigation, nor can we assert that the order of series within the groups followed a predetermined, logical plan. The phenomenon under investigation is both multiform and elusive, and we often shifted the course of our work to follow up new cues as they appeared.

The general instruction given to the *O*s read: "You are to describe what you see. Your description will be fourfold, including a characterization of (*a*) the figure; (*b*) boundaries of the figure; (*c*) contents of the figure; and (*d*) background-differences in and out of the figure." We shall indicate any change made in this instruction in cases where we found such a change necessary.

### (*A*) *Experiments with the Stereoscope*

For these experiments we used; (1) the ordinary Brewster-Holmes Stereoscope; (2) the Zeiss Precision Stereoscope; and (3) the Ludwig Tropostereoscope. We performed these experiments in a dark room, where the light which came from a reflector containing a 200 watt Mazda bulb filtered through a Gage 'daylight' glass. The reflector was fastened to a standard behind the *O*, at a height of 2 m. from the floor. Before allowing them to start upon the actual experiments, we trained the *O*s in stereoscopic vision by means of the Hausmann stereograms.[18]

#### (1) *Experiments with the Brewster-Holmes Stereoscope*

*Series a.* Each one of the 6 slides in this series showed a truncated cone or pyramid outlined stereoscopically in black upon a gray background, of which the texture varied from a very smooth paper to a cardboard which had been roughly scraped with a knife. This series was presented 3 times in serial order, beginning with the roughest background (Slide 1). If, as von Frey says, the flecks in the empty space are a function of the background, then we thought that we should receive reports of a snowy, flaky filling over the rough background and a fine dust over the smooth background; that is, the size of the flecks should vary as the roughness of the background varies. The reports should also indicate the presence or absence of a glassy medium in addition to the flecks. The *O*s were: A, Bi, By and H.[19]

We asked for a characterization of the figure only to determine the attainment of the proper stereoscopic effect; therefore we shall not include these reports in our results. In the discussion, S refers to bounding surfaces, C to content of the figure, and D to the background-differences; and the number to the slide in reference to which the report was made.

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[18] *Dr. W. Hausmanns 26 Stereoscopen-Bilder zur Prüfung auf binoculares Sehen und zu Uebungen für Schielende*, Leipzig, 1913.

[19] Dr. L. B. Hoisington (H) and Dr. H. G. Bishop (Bi) were members of the department of psychology; Dr. G. Adams (A) and Mr. F. L. Bixby (By) were graduate students in the department.

*Surfaces.* A (1) did not see the truncated pyramid. She reported, however, that the large square, which would have formed the base of the pyramid, was "glazed." . . . "It looks like isinglass." (2) In the one case where this was seen as a truncated pyramid "the large square is glazed, and the glaze forms the sides." In the other two presentations, "the small square is in front of the large square. It is more isinglassy than the large one. I can't tell whether the glaze belongs between the two squares or in the large one." (3) "The small circle is transparent; the sides are dense and glassy looking with streaks and splotches in them." (3) "The small circle is glassier than the rest. It is flaky and rough, but the glass is over everything and has the character of the background in it." (4) "In indirect vision, the sides are glassy, transparent and not of the same substance as the contents. The sides dissolve and disappear when I look for them. The small circle appears glassy and not transparent. The big circle is glassy, but the little circle is more so." (5) "The sides are more transparent than the small circle, which is more glassy-looking." (6) "The small circle is filled with something transparent, jelly-like, flaky, not definitely streaked. I don't know how far back it goes. The transparent glassy stuff has the character of the background in it in cracks and ice-splinters."

Bi is unable to find any surfaces for (1) and (2). (3) "A conical sheet of some kind links the two circles together, yet when I look for it I can't distinguish it from this glass mass." (4) "The small circle has a rigid, dancing, glassy surface that is a little blurry. The area of the small circle is like a transparent after-image." (5) "I think of the glass and the small circle as having a front, but I don't see any." (6) "I can force a glass surface to stand out."

By for all 6 slides reports: "Casually the sides and the bottom are made of the background. Critically the sides are made of glass." The surfaces are "a thin film of a shiny, shimmery, transparent, colorless stuff." In all except (2) "the sides appear to reflect light, like glass." Except for (5) "the base of the figure is made up of the color and texture of the background." The influence of the background on the glass is remarked in (1); "the shimmery stuff seems conditioned on the texture of the background. It covers everything." (4) "The surfaces are waves of colorless, transparent stuff. It's like little holes in the air." (6) "The glass is more markedly the texture of the card projected to fill the sides and the front opening. The figure is entirely surrounded by a film with very fine waves in it."

H (1) "The gray of the background seems to fill up the sides, and yet at the same time it is back. It does not fill up the front end." (2) "The gray lends itself to the sides, yet they are transparent." (3) "The sides are dense, crystalline, transparent, and glassy. The gray lends itself to the sides, yet seems to belong back." (4) "The sides are solid, transparent, and glassy. The gray lends itself, but it is certainly beyond." (5) "The small circle is perfectly transparent. The gray is there as though reflected in the hard, smooth sides, and seems to give a little of its character to the cone." (6) "The gray seems to form a complete surface, yet there is something beyond it. The background lends itself to the sides, which are glassy-like, hard, dense and transparent. It lends itself more definitely to the sides than to the end. It is impossible to be sure you see through the side at all. It is more like a reflection, because the side is glassy and smooth; yet you get this gray, which is rough and beyond."

*Contents.* For A the content of the figure is "glassy" or "icy", but "as isinglass is glassy. 'Glass' itself is too clear." (3) "The glassiness takes on the rough character of the background." (4) "This glass is clearer and more like ice than the others." (5) "A transparent jelly, without the color or shake of jelly." (6) "A soft glassy thing which holds the small circle out in front; flaky and not definitely streaked." *O* repeatedly states that 'glass' is not a good descriptive word. (5) "Glass bothers me as a descriptive word because it is too clear. Ice is better, because you can see ice and see through it too."

Bi reports all the content as "glass, which the background seems to affect, though I don't really think it does." It is compared in quality to "dim high-lights on slightly tarnished silver," "a hard, glinty, glittery, sparkling, transparent stuff." (3) "The glass sparkles. It is as if the texture of everything were fine instead of coarse. Since you see the background through the glass it has the effect of making the glass coarser or finer, but it only seems to. The glass is a very irregular crystalline formation, like zinc crystallized on iron, or like heat waves caught." (4) "Hard and unchanging and resistant like ice." (5) "The background is finer but the texture of the glass is not, it is constant." (6) "If I were less careful, I'd say this was a finer texture of glass. It is like seeing the bottom of a stream through ice. It films the background."

By reports in every case: "The glassy stuff has the texture of the background in it." The glass is "shimmery, transparent, colorless, wavy." (2) "The waves are finer and seem to be part of the texture of the thing." (4) "The glass inside seems to be more wavy than the sides." (5) "This shimmery stuff fills the cone. It waves up and down like a frozen shimmer. It is a colorless transparent something and seems to be texture even more markedly; like bubbles or fine holes punched in the air." (6) "The filling is a gelatinous substance. It has fine waves running crosswise like the grain of the background running that way."

H finds that "the filling is some sort of a transparent solid medium, yet the gray seems to lend itself to the body of the cone." "The gray belongs to the figure, but seems to lend substance to it." In one case (6) H reports: "The texture of the background is like the texture of the pyramid itself," and "cues for solidity are some bits of black quality in the figure."

*Background Differences.* A reports that "the background outside of the larger circle is a rough surface. There is no background inside the figure."

Bi says: "The background seen through the glass appears blurry as if out of focus." (3) "It makes the background look as warm air makes it look, only this doesn't go in waves, it is static."

By "The background inside the figure is smoother and lighter in tint than that outside."

H "The gray background shows sharp shifts from dark to light quality. The background through the sides of the figure is more uniform. There are still variations, but they are smoothed out. The shift of brightness is not so sharply defined. The gray seen through the little end is the same as that around the outside."

This was the first series taken, and it is evident that the *O*s are groping for descriptive words. We note (1) that all the *O*s saw more or less of what we may call surface glass and content glass; (2) that, curiously enough, no *O* directly reports dust particles or flecks, though we might perhaps infer their presence from A (S 3,6), (C 3,5,6); Bi (S 4), (C 3,5,6); By (S 1,4,6), (C 2,4,5,6); and H (S 6), (C 6); and (3) that the glass of the figure in some way has an effect upon the background which is seen through it. For A and Bi the sides are elusive; for By they differ with casual and with critical observation; and for H they are transparent yet somehow gray.

*Series b.* We introduced small crosses or circles as points of reference[20] upon the slides of Series *a*, which we presented in haphazard order. The *O*s were: A, Bi and H.

*Surfaces.* A (4) "Indistinct." (6) "You assume that there are sides holding this glassy stuff in."

Bi (2) "Nothing joining the circles, but I can read in a conical surface which isn't there." (6) "A hint of a conical surface, not visible, but somewhat in terms of eye-kinaesthesis."

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[20]H. v. Helmholtz, *Handb. d. physiol. Optik*, 1896, 917f.

H (2) "The front surface is hard, glinty, clear, more transparent, more alive, and less gray than the sides. The sides are slightly grayish, clear, hard, and not perfectly transparent."

*Contents.* A (3) "Glassy." (4) "Jelly-like."

Bi (1) "The whole slide is covered with glass so that there was no content proper for the figure." (4) "The glass is lustrous, extremely rigid, it blurs the background, and it diminishes outward to the end of the slide."

H (2) "The figure is solid. Some dark spots seem to float in the medium." (5) "Like the sides." (6) "Glassy-like and transparent."

*Background Differences.* These differences were reported as like those in Series *a*. Except where there is a change in the nature of the report, we shall not include this division in our future discussion.

For two *Os*, A and Bi, the presence of a surface glass is doubtful, while H finds it much as it existed in Series *a*. There is a glassy content, but again there is no definite mention of flecks, except for the dark spots which H reports as floating in the medium (C 2).

*Series c.* In order to determine the effect of the tint of the background upon the glass, we used a single transparent colorless celluloid slide upon which we outlined a truncated cone stereoscopically in black ink, and behind which at a distance of 1.5 m. we set up a cardboard background, which we varied. We used Bk, W and gray cardboard. The *Os* were A, Bi and H.

*Surfaces.* A (1) "Indistinct."

Bi introduces a 'fog'. (3) "The small circle is full of fog, and the conical surface is a fog (made of the background) that stands at the proper angle to be a cone."

H (1) "The front surface is hard, glinty, clearer, more alive, less gray, and more transparent than the sides." (2) "The sides are slightly grayish."

*Contents.* A (1) "Seems frozen." (2) "Whiter, stiller, and harder than the background; jelly-like." (3) "Not slick." The word 'glass' is not mentioned.

Bi (2) "Filled with the fog that makes up the background. The fog is shaped like a bowl. The inside volume is impenetrable. It's like a snow cone set in a snow background."

H (1) "Transparent; the same as the sides."

The change of tint itself apparently does not affect the figure, while the nature of the background may do so. The glass is not present for A and Bi. A sees a white jelly-like figure, with indistinct sides, while for Bi it is a foggy volume. The figure for H is unchanged.

*Series d.* The stimuli in this series consisted of a set of stereoscopic views; St. Martin's Cross, Iona; Ehrenbreitstein, on the Rhine; The Bold Axenstrasse, Lake of Lucerne; Ruins of St. Werner; and the Ruins of the Granite temple, the Sphinx, and the Great Pyramid. The instruction read; "You are to describe what you see, noting particularly the space between things." The *Os* were H, Bi, A and By.

A describes the space between the objects as "glassy." (1) "Like something frozen hard in between things, as if the things were set in ice." (2) "The glass isn't so clear that you can't see it, and it makes the third dimension more prominent than it would otherwise be." (3) "The glass holds the figure, which may be flat, apart."

Bi (2) "The hard glassiness fills up the space. The space is glittery." (3) "Grayish toward the back." (5) "Like a perfectly clear gelatine, where the only quality you get is high lights."

By (2) "The whole thing is very glassy, as if the picture were set in a glass like a paperweight." (4) "Very glassy. I can see the shining of light and imperfections. This may be in the lens." (5) "I can't say what the space between things looks like."

H (1) "The space is most definite in the foreground, where it is glassy, clear and neutrally colored. It has no definite front boundary." (3) "Bits of light and dark gray, localized between myself and the object looked at, give body, substantiality, reality to space." (4) "I don't believe I see anything in particular. It's space, good tridimensionality, but I can't say that I see anything between the things."

The *Os* indicate a wealth of glass. From A (1,2) and Bi (1,2) the presence of flecks may be inferred, while By mentions imperfections (4), and H attributes the reality of the space to bits of dark gray (3).

*General Summary*

The reports are very consistent. Except for A and Bi in Series *c*, all the *Os* see glass as content and as surface, although both A and Bi find the sides difficult to observe. The *Os* do not report flecks directly, but we may infer their presence in numerous cases.

(2) *Experiments with the Zeiss Stereoscope*

*Series a.* This series was a repetition of Series *a* with the Brewster-Holmes stereoscope. The construction of the instrument allowed us to add three finer backgrounds; a piece of transparent celluloid in the substance of which were small specks, placed upon the milk-glass plate which is part of the equipment of the Zeiss instrument; the milk-glass plate itself; and a glass vessel filled with milk. The imperfect piece of celluloid, upon which a cone was outlined stereoscopically in black ink, gave a background which was more uniform in texture than the smoothest paper. More uniform than this was the milk-glass plate; while the most perfect background of all, as far as uniformity was concerned, was the glass vessel of milk. With the last two backgrounds the cones appeared in outline on perfectly clear colorless celluloid. The stimuli were presented in serial order to two *Os* (By and Bs) and in haphazard order to one *O* (Bi).[21]

*Surfaces.* Bi does not see a surface. In every case he reports: "The surface is there by implication, but I don't see any sides."

By (1) "Not directly observable; a filmy, colorless, rather coarse-textured thing that's like the black inside a black tube, because, when you look at it to describe it, it's all around where you're looking, but not where you're looking." The surfaces differ only in texture; (2) "like frozen waves, a finer texture than the first;" (6) "only slightly discernible;" (8) "a colorless, transparent, textureless medium."

Bs (1) "The sides are grayish-looking; rough and fuzzy surfaces." (3) "Glassy; about transparent. I seem to see through the sides, and I see this other stuff through them." (6) "Like the background to a certain degree, but transparent and more delicate." (7) "You look through the glassy, smooth, transparent sides to the filling." (8) "The side is sort of glassy-looking; slick-looking like blanc-mange, and you look through it to the filling."

*Contents.* Bi does not report a content of the figure, but a content of the entire visual field. Except for (8) and (9), the report for (3) is typical: "The glittery glass is everywhere in front of the background. The black lines are like wires set in the glass. The glass is very rigid, and it is thicker through the small circle, and gets thinner out toward the edge of the field." "The glass is blurrish, like dust in the air, but it isn't that, though it reminds me of it." The reports for (8) and (9) are alike: "No glass; a cone-like thing simply suspended in a mist, and filled with the mist."

By (1) "The same sort of thing as the surfaces, except that it's a coarser texture; fills up the whole inside of the figure, which is a hollow shell." The content differs from slide to slide only in texture. (3) "The hollow shell is filled with a colorless mist of finer texture than the other figures;" (4) "a

[21]Miss F. Burns (Bs) was a graduate student in psychology.

mist like the sides; has little drops in it; denser and more compact than the sides, and almost fluid or bulky, instead of filmy;" (7) "a shell filled with a transparent medium like the surfaces; always a marked difference between surfaces and filling; the same general stuff, but the inside is bulky or filmy, while the outside is a very smooth surface;" (8) "seems to have a very fine texture about it;" (9) "a colorless medium, which is smooth, bulky and transparent."

Bs (1) "Like the glass of a heavy paper-weight; thick, glassy, air-like. The fuzzy stuff (of the background) makes it have an icy appearance, as when you see through a thin icicle, or like what's between the surface of the water and the image of an object in it." (2) "The same glassy stuff but not so thick or heavy as before; less dense; like ice that's full of cracks, but is not falling apart." (3) "The same glassy stuff; concentrated through the center, and thinner through the sides." (4) "The same glassy stuff, but more transparent, clearer, brighter, than before; like looking into a clear liquid." (6) "Not as heavy or as thick as the others; less bulky." (7) "Just like clear ice." (8) "Grayish; looks like jellied dishwater." (9) "Like the other one, only it's white and looks heavy like blanc-mange."

*Background Differences.* Bi for (8) and (9) reports no background, only a mist. In all other cases he reports, as in (2): "The background is behind the big base. It is not blurry itself, but is seen through a blur;" or in (6) "The blur is so great, like a shimmer over a hot stove, that the background isn't localizable. The blur takes place everywhere, but is so marked in the little circle that you don't even attempt to see the background."

By (1) "Through the figure the background appears filmy and a little lighter than the outside background. You see this gray film through a transparent frozen-wave medium. The outside is a rough surface." This description is general for this *O*. The only change is in the degree of roughness of the surface, which corresponds with the degree of roughness of the slide.

Bs (3) "What I see through the sides is lighter and more delicate in texture than the outside background. Through the center, the background is darker, heavier and grayer than through the sides. It looks as if there was a glassy stuff over it, when I look through the center." This description holds for all observations.

*Series b.* In this Series we kept the slide constant, but altered the illumination by changing the level of the light. The *O*s were Bi, By and Bs.

*Surfaces.* Bi (1) "I can more or less see the curved surface or not, as I please. The glass extends out in a shapeless mass, or I can cut it down more or less to the surface of the cone." (3) "A little harder to see a surface this time."

By (1) "A transparent colorless material. Texture seems to be part of the background, rather than part of the sides, which are smooth and shiny." (2) "The same kind of transparent substance, smooth and rather thin;" (3) "colorless transparent stuff; very thin and smooth."

Bs (1) "Glass; transparent, and you see the stuff in the cone through them." All of Bs' reports are like this one.

*Contents.* Bi (1) "The whole slide is full of glass and the glass is denser in the middle; denser in the sense of blurrier. It's more conspicuous there." (2) "Everything is glassy, but not so glassy as before. The greater amount is in the middle again. This one fogs over, loses its glass and becomes a vapor or a fog." (3) "Like the last one, though it fogs more readily."

By always described the figure as "a hollow shell" filled with a "transparent stuff like the sides." In (2) this content is "less dense than in the first figure," and in (3) it is "still less dense."

Bs (1) "Glassy or icy-looking; thicker than the stuff in the sides; like what you see in paper-weights." (2) "Not quite as bulky or thick as the

other one. It looks like frozen air. It has the smooth, clear appearance of white gelatine." (3) "Less glassy-looking, less frozen looking than before."

*Background Differences.* As reported in Series *a*.

The surface glass is unchanged by a change of illumination. The content, however, for Bi becomes less glassy and more like a fog; for By less dense; and for Bs less glassy or frozen-looking, as the illumination decreases in intensity.

*Series c.* With the instruction "You are to describe what you see," we presented white slides (cardboard of very fine grain and dull surface) which we had peppered with minute dots. In some cases the dots were arranged in a pattern, while in others their arrangement was left to chance. The *Os* were Bi, By and Bs.

Bi (2) "It's like looking down on a city at night. The dots are all scattered around, some near, some far, in a glassy volume. It's what looking down on snowflakes from above when they're falling would be, only I've never done that and, of course, this doesn't move."

By (1) "The dots float around in a film, in front of the white quality, which lies away behind them. They are not all in the same plane." (3) "Instead of being a white background with black dots on it, it's black dots floating away above the white. No dots are in the white. The white is still beyond that."

Bs (2) "Some dots seem closer than others." (3) "They are not all in the same plane, but one stands out higher or lower than the one next to it."

The dots, which appear in different depths, are said by Bi and By to lie in a transparent medium. Bi calls this a glassy volume, By a film; there is therefore, something between the dots. Bs, on the other hand, does not mention a medium of any sort, but merely sees the dots in different depths.

*Series d.* Following von Frey, we photographed a number of simple objects and presented to the *Os* (Bi, By) identical instead of stereoscopic pictures. We used the simple and complex geometrical forms which are described later under skeleton forms,[22] transparent glass paper-weights, a rectangular chalk-box, a small toy pig, and a small toy horse. The pictured objects measured not more than 3 cm. along the longer side. There were 24 slides in all, 12 printed upon dull, 12 upon glazed paper. The skeleton forms appeared outlined in black on white and on gray backgrounds. The paperweights, the box, and the toy animals stood upon a light gray surface before a dark gray wall. Shadows were present only in the pictures of the toy animals.

Bi. This report (rectangular parallelopipedon) is typical of the reports for skeleton forms: "The divisions of the instruction don't seem to apply here. The figure isn't tridimensional, but flat. It is set in the glass, which you would have just as well without the figure. It doesn't make up the figure." For the solid forms the report was (pig): "A pig in ice; a lot of glittery glassiness there, but it doesn't belong to the pig. He is set in the glass, though he doesn't belong to the glass. His sides are made up of dark and light shading." Bi does not report a difference in glassiness for the dull and the glazed slides.

By. The following report (triangular pyramid) is typical for the skeleton forms: "An outline figure; without surfaces or content; like a picture of a figure; not tridimensional. The whole field of vision is filled up with a glassy stuff which is quite dense. None of this glass is inherently bound up with the figure. The glass is very fine and colorless." (Horse) "Between me and the animal there is a lot of glass which is very dense, colorless, al-

[22] See p. 271.

most lustrous, with little white flecks suspended in it. But the horse looks like a picture, not like a tridimensional horse. The glass doesn't belong to the figure." The glass of the dull background is described as "lighter in shade and not so thick, less body to it, than with the shiny background."

Although the figure itself is flat, there is no lack of glass in the field. The *Os* agree that this glass is present over the visual field without reference to the figure, which is set in it. By definitely reports flecks, and their presence may again be inferred in the "glittery" glass of Bi. The character of the glass over the dull and the glazed backgrounds is unchanged for Bi, but By finds a difference.

*Series c.* We presented to the *Os* (Bi and By) some of the slides of Series *a* and asked them, when they had reported the "shimmery or glittery" glass, to describe what happened when they teased this glass with a fine steel knitting needle (size 20), held in the right hand.

Bi "It doesn't change it. It looks the same. The needle is just in the glass."

By "It destroys the whole figure, because I get to looking at the needle. I don't think it destroys the glass though. I think the glass stays."

Although the figure for By is destroyed, for both *Os* the glass itself is apparently unchanged. The needle is simply thrust into it.

*General Summary*

These reports are very much like those of (1). Glass is present for all the *Os* in all observations, except for Bi and Bs, Series *a* (C 8, 9) and Bs, Series *c*. Bi and By do not actually see surfaces, though for By they are 'there' but not directly observable, and for Bi they are 'there' by implication. Bs reports a transparent side. That the background affects the content is again indicated by Bi, Series *a* (C 8,9) and Bs, Series *a* (C 8,9), as well as in Series *b* by all the *Os*. Again there is no direct report of flecks.

(3) *Experiments with the Tropostereoscope*[22a]

*Series a.* We used red glass discs upon which pairs of white circles were so placed that with different settings of the instrument a truncated cone of varying height appeared. There were 8 positions in which the cone pointed toward the *O* and 6 in which it pointed away from the *O*. The longest cones tipped slightly up or down. The *Os* were A, Bi, By and H.

*Surfaces.* A. Whether the cone is truncated toward or away from her A's reports are in general the same. (2, truncated toward *O*) "I can't say much about the sides. They must be there, but I can't see them." (6, truncated away) "Something holds the two ends together, but I don't know what it is."

Bi (1, truncated toward *O*) "The natural way seems to be for the red to fill up all the space between the two circles." (4) "As if I took tar and stretched it out with a little ring. Then the small circle would be flat, and the other any near-conical surface." (8) "The boundaries are like rubber, if you had a little ring in it and stretched it out." (1, truncated away) "The red bridges the difference between the two circles, as if I had a curtain and just pushed it back." (6) "A dense red film forms the walls of the tube."

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[22a] Since this work was completed, we have taken some observations with the Sanford model of Helmholtz' telestereoscope. We found nothing new, and therefore did not extend the observations to a complete series. The instrument does not furnish compulsory conditions for the perception of glass.

By (3, truncated toward *O*) "The sides are a colorless film. You don't seem to see them as much as you are aware of them." (4) "Now I get a very dusty sort of thing, a film. It is boundary, and is fine and small. Dust is a bad word." (1, truncated away) "I get two circles and a red film which is everywhere between the circles."

H Truncated toward *O*, the side is generally described as "quite sharply defined"; (3) "crystal-like"; (1) "light and flinty." (2) "The front surface is glassy." (6) "Transparent or nearly transparent." (4, truncated away) "The sides are very definite on the inside, but the outside is unknown to me. I see the inside, not the outside." (5) "Rather hard and glassy-like."

*Contents.* (1, truncated toward *O*) "The red is at the bottom and you see into the figure." (2) "The content couldn't move, it is opaque." (5) "Jelly-like, but softer than jelly." Truncated away from *O*; "No content."

Bi (4, truncated toward *O*) "The red shapes itself into a film cone." (6) "A vaporous, thin, sparse film." (8) "The film is not hard or glassy, but soft, steamy and vaporous." Truncated away from *O* the figure is empty. (4) "No sparkling glass here, but a fluffy film color, that looks soft instead of hard as the glass did."

By (2, truncated toward *O*) "One minute I think it is filled with a colorless substance, and the next I think it is filled with a red substance. The filling is a smooth, transparent medium, without those little waves in it." (4, truncated away) "No figure. I get two circles in a red film which is everywhere between the circles."

H. Whether it is truncated toward or away from him H sees a solid figure. (1) "Slightly pinkish perhaps, though it might be perfectly transparent with the red beyond." (2) "The figure reflects a color rather than being colored itself." (6) "Glassy-like; a bulky mass." (3, truncated away) "Lively, and I get this red showing through it."

*Background Differences* are not reported. The background for all the *O*s is simply a film.

Only one *O* (Bi) definitely reports a colored filling. A and By again find the indefinite colorless side, and H continues to see transparent, glass-like bounding planes. For Bi the sides are a red film. We note that Bi (C 4) remarks the absence of glass and that A and By do not report its presence, while H describes both surface and content glass.

*Series b.* We used a combination of two metal discs each of which had a 3 mm. strip cut out along a radius, and two blue glass discs, to get a blue line upon a black background. With different settings of the instrument, the line varied its position in relation to *O*. It tilted away from or toward in 4 positions and in the fifth position became a horizontal bar. The *O*s were A, Bi, By and H.

*Surfaces.* A and Bi do not see a surface. "The blue line is a film." Its edges are for A (1) "not sharp; blurred but easy to find"; (4) "soft"; and for Bi (3) "blurred, but fairly sharp"; (4) "the part near me is sharper than the part away from me."

By reports every time that "the line is distinctly surface." (1) "The outlines are not well defined. They are blurred by the suggestion of a mist which is over the thing." (4) "The near boundaries are well defined, and the near end is more vivid in color. But the far boundaries are blurred. It fades away into quality almost without shape. The back end is soft and mellow."

H (1) "As a bar of light, the front surface is indefinite; as a rod it has a pretty good, fairly definite front surface." (5) "The boundaries around the thing are fairly sharp, but a little fuzzy. There is a sort of halo around it."

*Contents.* A (2) "A blue luminous stuff." (4) "It is vapory, spongy, and it has no dimensions."

Bi (1) "The blue film." (3) "The blue film is more localizable than the black, because it doesn't have any room to get bulky."

By (1) "No contents. The line is just suspended in blackness. There is a colorless shimmery mist through which you look at the line, but you can't see whether this mist is a wall or has depth."

H describes two contents. As a rod (1) the content is "a rather luminous glassy mass"; (3) "hard, smooth, glowy and glassy-like"; (4) "blue, quite dense, translucent, and glassy-like." As a bar of light (3) the content is "softer and fluffier than when it is a bar"; (5) "less obviously tridimensional."

*Background Differences* as in Series *a*.

A and Bi do not see a surface, and H finds it "indefinite" or "fairly definite" (S1). The *O*s (except By, for whom the line is a flat surface and therefore without content) seem to agree as to content: *i. e.*, the non-dimensional stuff of A is fundamentally the same as the film of Bi and the fluffy filling of H. H, however, reports a change to a harder content under a certain perceptive attitude (C 3). H is the only *O* who reports glass (C1,3,4) though A says that the content is "luminous" (C 3) and By mentions a "shimmery mist" (C 1).

*Series c.* We used 3 yellow dots in the same manner as we used the blue line in Series *b*. The positions of the row of dots in regard to *O* were identical with those of the line. The *O*s were A, Bi, By and H.

*Surfaces.* A (2) "Too glowy to be a surface, yet there is a suggestion of surface there." (3) "A film, but almost a surface; too cloudy and too luminous to be a surface."

Bi (5) "The yellow is a film; without form except that it is flatter, more localizable, and more surfacy than the black."

By (1) "The dots have a smooth white surface." (5) "At first the dots are well defined, surfacy, lustrous dots, but then they grow dimmer and less well defined. Then they are patches of white without lustre, seen through a mist." (6) "In front of each dot there is a colorless film which isn't there at first. I don't really see the film, but the dots get ill defined as if you were looking at them through a mist or something."

H (3) "The dots are flat bits of brightness." (5) "The dots have a fairly definite surface, which is hard, glassy-like and dense." (6) "The boundary between the yellow and the black is fuzzy."

*Contents.* A (1) "Luminous yellow." (2) "I can't see into it as well as I can into most films." (3) "A film, but almost a surface; vaporous; looks like light shining through it."

Bi (1) "A yellow film fills the holes." (2) "Except for the quality, it is just like the black." (5) "The yellow is without form."

By (1) "I can't see any content. It's as though the spheres were made of paper."

H (1) "A glowy mass." (2) "It has the density of a bulk color, but I can't see through it." (4) "It ought to be bulky but it isn't, because though it is bulky it isn't transparent;" "(3) "translucent and alive; jelly-like, fiery and brilliant, but not a flamey, flashy, leaping flame"; (6) "glass-like."

Only one *O* (By) does not see a content. For A and Bi this content is a yellow film, with a hint of surface; for H it is a glowy mass with a definite surface. Both content and surface are glassy for this *O*. By sees only a smooth surface which becomes ill-defined, suggesting to him that there is a mist over it.

*General Summary*

In these experiments only one *O*, H, reports glass. By mentions a mist in Series *b* (C) and Series *c* (S) which might be considered glassy, in that he says it "shimmers." In the same way the "luminous content" of A may also indicate the presence of glass.

### (*B*) *Experiments with Skeleton Forms*

The forms which we used in these experiments were (1) a set of cones and pyramids, (2) a set of simple and of complex geometrical skeleton forms, and (3) a set of irregular ridges. To avoid as far as possible the formation of contrast lines, we painted the outlines a dull gray. We performed the experiments in the dark room where we suspended the figure by means of an invisible thread from a carrier. The *O* viewed the object through a 25 × 35 cm. opening. Except in those cases where we wished to determine the effect of changed illumination, (1,*h*;2,*b*;3,*a*), the light—a 200 watt Mazda lamp, reflected through a Gage 'daylight' glass which was faced with a double sheet of tissue paper—stood in front of the figure, toward the right, and well above it, so that the figure was uniformly and moderately lighted.

#### (1) *Experiments with Skeleton Cones and Pyramids*

*Series a.* We had 5 circles and 5 squares made of 1/16 wire. The diameters of the circles and the sides of the squares ran from 10 to 30 cm. by 5 cm. intervals. Thus it was possible to form a truncated cone or a truncated pyramid by hanging in the carrier one circle in front of another circle or one square in front of another square. The length of the figure and the degree of truncation depended upon the size of the circles and squares and their separation, which could be varied from 15 to 90 cm. *O* sat 4.5 m. from the opening in which the figure appeared, and 5.5 m. from the base of the figure. We presented 6 figures, with a black valvet background. A, Bi, and H acted as *O*s.

*Surfaces.* A (1) "The sides seem to be there until you look for them, when they dissolve so that you can't find them at all; a soft film, but I can't say whether it is transparent or black. The film is static, but being static doesn't make it more definitely where it is, but it seems there between the circumferences of the two circles and I can't get away from it." (3) "I can see where the sides should be. The small enclosure is almost coal black. It looks like very dark liquid or clear rock. It's congealed." (4) "The sides are lighter and more filmy than the smaller square. This square is hard in that it looks glazed, though it's a kind of film, too." (6) "I can't tell whether the sides are film, or whether the whole thing is film." (2) "The small circle looks like the mirrored reflection of black velvet which shows the glaze of the mirror and the velvet too."

Bi (1) "The sides are a sort of black film, as if the black film of the background were pulled up to the small circle." (3) "As formless as an afterimage." (5) "In the little circle it's like looking into a black box." (6) "Most of the time the two bands are tied together by a dark filmy grayness, so that a film lies outside the little circle, and inside the big one. Though it isn't a surface, it seems to be stretched there."

By (1) "You would say that they are black, but when you look there is no side, yet it has the meaning of cone." (2) "The sides are black without surface or texture, they are just extent." By twice reports (1,2) "no front plane"; and except for one other figure (3) he says: "A mist comes up and fills the window so that the whole thing lies behind the transparent shimmery stuff, and I can't tell about anything on account of this." In (3) he sees "the front end more surfacy, blacker, and more shiny than the other surfaces."

H (1) "The sides are a practically transparent gray." (2) "The surface belongs more to the sides than to the top and bottom. The side boundaries are very definite." (3) "The front surface is less certain, less clearly seen

than the side surface, yet something in the front end gives a hint of surface." (4) "It has a definite beginning-place on the outside, and the front is more absolutely transparent than the side surface."

*Contents.* A (1) "As if the whole figure were made of a thick ink that had congealed." (3) "The thing is not transparent but opaque, and yet not perfectly opaque; misty opaque so that I can see inside a certain way." (5) "The contents seen through the smaller base are bulky, almost liquid looking. Seen through the sides they are doubtful. It might be a hollow figure covered with film."

Bi is not sure of the content. (6) "Blackness, like looking into a black box"; but (5) "it may be hollow or it may be filled with black, but I don't see anything."

By (1) "Just blackness, as if you were looking inside a tube or cone." (2) "The black of the content is smoother than that of the blundaries; textureless and pre-dimensional." (4) "A cloud of intense black." (6) "It seems hollow, but filled with black."

H is very doubtful about the contents. (1) "I know nothing about the inside of the sides. It doesn't stop anywhere, and yet I see it. Perhaps it stops at the bounding surface." (2) "My impression is that the side stuff fills the whole mass of the figure, but I see it at the margin." (3) "The whole thing seems filled up, yet all I can see as quality belongs to the surface. It means filled, but I can't see it."

*Background Differences.* A always reports "a filmy surface unlocalized with reference to the figure." Bi always reports "a black film." By always reports "unlocalized blackness." H says that the background is not definitely localized. (1) "The background outside the figure has a vague hint of something more substantial, more real about it." (3) "Through the small circle the background is very black. Through the side it is blurred, but still pretty black as background." (6) "A perceptual divorcement of gray and black. The black is very black (background) though actually blurred over with the gray (figure)."

As in the stereoscopic experiments, A and By report the indefinite sides. Bi pulls the film up from the background, and H alone sees definite surfaces. There is a difference between the sides and the front surfaces, and the content is different for different *O*s, though Bi and By agree that it is blackness if it is there; and A sees the content differently through the sides and the smaller form. H is the only *O* who connects background and figure, and he notes the curious relation of the two in D 6. A suggests glassiness or iciness in the "glaze" and the "congealed surface" of the small form (S 2,3,4) and perhaps in "congealed ink" (C 1.) The transparent shimmery stuff which fills the window for By (S) and the shiny surface (S 3) may also indicate the presence of glass.

*Series b.* We used figures from Series *a* with black velvet, gray cardboard and black cardboard backgrounds. *O*s were A, Bi, By and H.

*Surfaces.* A (1) "I can see where the sides should be, but I can't see them."

Bi (1) "The small ring is filled with a black film, which is unlocalizable in that it is not a surface. It's cupped, too, instead of being flat." (2) "The gray is not observably in the ring, but it's not seen out of it either." (3) "The implication is that the film filling the small ring comes from the background." The only difference in the reports is that the tint of the film varies with the background. The "black film" in (1) is a "gray film" in (2). (3) "The sides aren't there as surface. It is like a cone turned out of a film that is so vague that it hasn't very definite limits laterally."

By. Again the only difference from report to report is in change of tint. (1) "As a whole the figure is a truncated figure with black smooth sides; but when I try to describe them, there is nothing there." (3) "A suggestion of

looking through some sort of a transparent medium which is observable on the sides of the figure and over the bases. It is shimmery, colorless, and of fine texture. It seems to move." (4) "This medium is not exactly a mist, but more a transparent film." "At times the front end is a shiny black surface, which is a very deep and intense black." This last report came only once, for (1).

H (1) "The front end is slightly grayish, but less grayish than the side. It is poorly defined." (4) "Less stuff given in the front end." Even with a black background the sides are reported gray. (1) "The sides are filled up better than the front end. The gray is certainly along that side." (2) "The gray of the side is a sort of bulky volume; a transparent gray which is sharply defined on the outside." (3) "The thing is gray and surface, and somehow the surface is dominant."

*Contents.* A (1) "A film. I think it is light gray, but I can't be sure, because I see the background through it." (2) "The film-content looks congealed like ice, though there is nothing glazed about it. The particles are held together, fixed; a frozen mist, softer than ice or glass; like transparent ice-cream." (2) "Through the small circle this film is glassier, harder, more jelly-like and bulkier than when it is seen through the sides." (3) "Through the small circle the film is like a liquid mass." (4) "Ice and glass are too smooth for the content. You see what is behind it through it."

Bi. The contents differ only with regard to tint. With a black background the filling is black; with gray, gray. (1) "The contents aren't there any more than the surface of the circle. The black is everywhere, except outside the conical surface." (2) "The gray filling is the smokiest kind of dense fog." (3) "Over the black I get a shimmery effect like a dance of heat waves, and over the rings it is like a lot of fine threads. It is wiggly and mobile and is the nearest I come to the stereoscopic glass."

By (1) "I can't tell about the content because of this mist across the front." (3) "It may be filled with black quality like a tube, or it may be just a plane front; you can't tell." (4) "It may be filled with a colorless, misty medium through which you see a black medium."

H (1) "Gray, which is cloudy-like, and lighter in some parts than in others; mobile and alive." (3) "Cloudy; I mean that the gray itself is denser and rarer in some parts, though the denser parts appear lighter in quality and tint." (4) "Certainly the figure comes to me as solid and the contents are furred. I am inclined to take it as a bulky mass, because all the gray that I see belongs to the figure and not beyond; yet all the gray is transparent."

A, Bi and By all have difficulty with the sides. A sees where they should be; Bi sees a film, but nothing as surface; and By sees the sides as gray, black or colorless, but when he examines them they disappear. Bi apparently fills the small form from the background from which, too, the side film takes its tint. By, when he sees the sides as tinted, also takes their tint from the background. In only one case, in one observation, does he find a surface for the small circle (S 1). H reports a definite gray surface which is not affected by a change of background; and the small circle is like the sides except that it is poorly defined and less gray. A does not know whether her film-content is itself tinted or whether it gets its tint from the background which she sees through it. For Bi, the foggy content, which is just as indefinite as the sides, varies its tint as the background varies. The mist which fills the window makes the content difficult for By to describe. He cannot say whether what he sees is really a bulk (which is either without tint or with the tint of the background) or a plane. The content for H is always gray, but in the same figure the gray is not uniform. A reports a kind of glass in which there are particles which are held together. It appears different through the small circle and through the sides. Bi reports

an approach to stereoscopic glass (C 3) with the same simile which he used in the stereoscopic observations. Somewhat like this is the shimmery medium which By describes, not however as content (S 3).

*Series c.* We used 8 skeleton figures, each one with part of the outline blotted out.[22] We obtained this effect by wrapping a section of the wire with a black velvet band. The bands were 2.5, 8.75, and 25 cm. wide. The *O*s were A, Bi, By and H.

*Surfaces.* A (1) "Something connects the two circles but I don't see it as boundary." (2) "The sides are like mist laid around the sides. I don't really see it as boundary. The front plane is almost like glass." (3) "Where the piece is cut out it is dull, heavy, and dark, quite different from the figure, which is light and soft." (4) "The planes are filled with something like gelatine paper." (7) "It is just there and I can't see where it begins and ends." (8) "If I take the figure as a hollow tube, the sides are filmy and gauze-like, but if I take it as a cone with a piece cut out of it I don't see any side plane." "The disc seems cut out of the small circle and makes this circle filled with something more surfacy but yet it is not quite a surface."

Bi (1) "Sometimes the sides are black and solid, sometimes they thin out and become less dense." (4) "The little circle gets so solid that it bulges. This solidifying does not apply to the content as seen through the smaller circle but only to the surface of the circle." (7) "Where the piece is cut out the contour of the cone surface is interrupted. It destroys that part of the surface, but the rest of the cone is all right." (8) "Nothing definite in the cut-out part except black."

H (1) "The sides are neutral gray in tint." (3) "Though a piece is out of the boundary, the side is uniform wiht adjacent parts of the cone." (5) "At the gap the gray breaks off along a fairly definite line, though there is not a definite line there. It fades quickly in a fairly straight line into black." (7) "If I don't fixate the cut part, the figure shows a gap as if this side were simply cut off. There is less of the gray in the front end than in the sloping surface, but it is of the same brightness." (8) "If the gap is filled in, the gray in that part is like the rest of the boundaries, but it is not a constant gray. It changes in amount, softness and density, and it is lively. When I get something like a glassy, flinty, hard, almost reflecting surface there, the gray is more stable and less transparent." (1) "The front end is a little harder and more glassy and not quite as soft or as suggestive of depth as the sides."

*Contents.* A (2) "I can't say what is behind the mist-sides, if anything. It is a solid cone, and I see into the content a little way. The content in the small circle is much more solid looking and more liquid looking." (3) "Through the small circle the content is almost liquid." (4) "I see solidity without seeing solid content. It is liquid and viscous." (6) "Just about filmy." (7) "A soft mist-like stuff, lighter than the background and the stuff in the smaller circle."

Bi (2) "I don't know about the inside, I see the outside, yet through the little circle I get the impression that I am looking into a lightless space." (5) "The little circle always has black up to the mouth of the cone." (6) "The black comes up to the little circle like night to a window."

H (1) "I take it to be solid yet there is the same old dilemma; I don't see the stuff inside the figure." (3) "The figure with the chunk out is just as much filled as the one with complete boundaries, only part of it is lopped off." (5) "Casually it is hollow. It solidifies in terms of the front surface." (8) "I never have seen anything inside the figure. I just take it to be solid, because of the transparent sides that I can't see any margin to."

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[22]We owe the suggestion of this variation of the experiment to Professor H. C. Warren of Princeton University.

It is surprising that the elimination of part of a boundary has so little effect on the figure. We rather expected that the *O* would look into the content of the figure through the gap thus provided; but in no case did this happen. The surfaces for A continue to be indefinite and misty, and the front circle again approaches glass. While the piece cut out makes the front form more surfacy, it still is not quite a surface (S 7). A reports a marked difference in the sides when she takes the figure as a cone and when she takes it as a tube (S 8). Bi departs from his usual report when he describes the sides and the small form as solid planes (S 1,4). H still sees the gray sides and the slightly less gray front form, to which he now ascribes a glassiness (S 1). H is the only *O* who sometimes fills in the gap in the side of the figure, so that there is a continuous surface; and even then, there is a difference between the grays (S 8). All three *O*s find that where the piece is cut out the figure simply does not exist. The reports of content are practically unchanged. A still sees the content differently through the small circle and through the sides. Bi again reports "blackness" although he is not yet sure that this is really content (C 2). H has his constant difficulty; he does not see a content, but because the sides have no inner margin, he takes the figure as solid (C 8).

*Series d.* In this series we used only one cone (a 10 and a 25 cm. circle with a separation of 45 cm. before a black background) into which we introduced a fixation point successively in the following positions: in the front circle; in the back circle; on the background; in the back circle which had a section blotted out by an 8.75 cm. black band, at the same time that the front circle had a section blotted out by a 2.5 cm. black band; and in the front circle which had a section blotted out by a 2.5 cm. black band. We added to the instruction "Please fixate the point which has been provided." *O*s were A, Bi and H.

*Surfaces.* A reports once: (3) "The sides look as though they should be glassy, as though I were seeing the background through them. It has a glazed character in spots, *i. e.*, it looks like glass and means glass." For the rest of the figures A says: (1) "I can't see the boundaries. I see where they should be." (2) "The sides look filmy, but they are hard to see as boundaries." (3) "The small circle is glassy. It doesn't seem to belong to the sides. It has the glazed stuck-together look of glass, not glass as a bulk, but as a thin sheet through which I see the fixation point." (4) "The small circle is seen as the surface of a solid. It is denser and more congealed than the side." (5) "Where the plug is cut out it is black."

Bi (2) "Nothing substantial links the two circles together; a transparent shell that lets the black show through it." (3) "Not side surface, but I can force a gray shell which is visible but not substantial." (4) "I can't quite see where the gray surface (the unsubstantial shell) meets the black, where the part is missing. Where the part is missing it looks like background and not like surface of a cone." (5) "A light and a dark surface meet (gap and sides), but it is not quite like a black insertion in the surface; more like a tearing out, and the blackness is there."

H (1) "The sides and the front end are gray. The front end is filled with gray which is thin, flinty, glassy, yet a little soft. It tends toward a bulk." (2) "The sides are flinty and dense." (3) "If I take it as a cone the sides are softer, fluffier, more filmy, and not so bulky or glassy as if I take it as a tube." (5) "With the fixation point the sides seem less suggestive of tridimensionality. They are more definitely gray, glazed surfaces. Where the circles are broken there is no gray, just a black background, which does not fill the gap but is beyond. The front end is filled with gray, but it is transparent; glassy, flinty, and dense."

*Contents.* A (1) "Filmy." (2) "I take it as a solid cone and it would be, only there isn't enough stuff inside to fill it up well. There is something there but not enough." (3) "Through the small circle it is hollow. Through

the sides it is filled with something glazed, transparent, and formless." (4) "I don't see it as a bulk though, it just fills in. The small circle seems to be the surface of a solid."

Bi. For this *O* the figure is filled only when the fixation point is in the front circle. (1) "A bulky figure made up of homogeneous black that has limits but not a surface." (2) "The fixation point hollows out the cone." (3) "The fixation point pulls the black back with it."

H (1) "It is variable; it is solid and it isn't." (4) "Most of the time it means solid, but I can't see any content. I see only surface gray, and I think it is hollow." (5) "All I see is surface, but it means a complete solid."

Except for the one case ($S_3$) where she reports sides that look like glass, A finds the same trouble in seeing the boundaries, though she sees where they should be. The small circle is again different from the sides and appears as a thin sheet of glass (S 4). Bi returns to the indefinite sides, with an unsubstantial gray shell. H still sees the sides and the front end as gray. His figure appears either as a cone or as a tube. As a tube, the sides are glassier (S 3). Although the gray surfaces (both sides and front end) are soft, they are also flinty and glassy. In one case ($C_3$), where the fixation point is on the background, A says that the figure is hollow through the small circle, while through the sides it is filled. She takes the figure as solid, though she cannot describe much of the content. Bi reports the indefinite blackness for film only when the fixation point is in the front circle. In other cases he finds that the point hollows out the figure. H is undecided about the content. He still sees only surface, and yet the figure means solidity. For all *O*s the cone simply does not exist in the place where a piece is blotted out.

*Series e.* We repeated Series *d* with a 10 cm. circle suspended 90 cm. in front of a 30 cm. circle before a black velvet background. The *O*s (A, Bi and H) sat at a distance of 2 m. from the opening in which the figure appeared.

*Surfaces.* A (1) "I see where the sides should be, but I really don't see them as plane surfaces. The small circle is a film, but it is hard, smooth, more definitely in one plane and more suggestive of a solid." (2) "The small circle is transparent and glassy." (3) "If I look through the small circle the cone is a hollow cone of glass. It is good glass, too." (5) "A dense, dark, film strip stretches from the small circle to the large circle. Still, I don't see this as a side surface." (6) "The sides are filmy, vapory; not distinct at all." (7) "The small circle is filled with something really glassy; transparent and slick like glass." (10) "Where the piece is out there is a black blotch as if the background came up and filled the side in."

Bi says that the sides are either gray or black. He reports the presence of a "gray halo" for half the presentations. (1) "The sides are a film of blackness." (2) "The side wall is a black shell, like paper in shape only." (5) "There is a gray halo that forms a surface except over the gap. The front circle is almost full of black." (8) "It is a gray shell." (9) "Where the ring is broken there is just blackness, like a piece torn out of the wall."

H (1) "The sides are not definitely seen. A certain amount of gray lies in between the circles very indefinitely. It seems at times to go into the back circle." (3) "The sides are grayish but not uniformly so." (5) "The front end is definitely filled in with gray. The sides are not very thick neutral gray, but they mean a definite surface." (6) "With sharp fixation there are no sides, but with less active fixation there is a certain amount of gray filling, which takes on a directional aspect more or less between the two circles." (8) "There are hints of gray in the sides; a gray that is not dense or hard, but fluffy and soft. By 'hint' I mean that the relation between the two circles is a meaning of relation rather than a matter of seeing. If I take it as a tube wall it is more definitely and uniformly gray. It is harder,

glassier, less fluffy and more definitely in a plane." (10) "It is slightly cloudy and not very definitely in a plane, but it carries the meaning of side." (9) "The big circle is more or less filled out in its own plane; just a blur of neutral gray."

*Contents.* A. Where the fixation point is not in the front circle *O* says: (3) "Through the small circle the figure is hollow. Through the sides the content is filmy, glassy, hard, and transparent." With the fixation point in front: (1) "Through the small circle the content is filmy, more suggestive of a solid, smoother, harder, and more definitely in one place than through the sides." (5) "Through the sides it is filmy and unlocalized." (6) "Through the small circle it is like the sides but not so soft; more sheen to it; a sort of slickness." (10) "Vapory and filmy."

Bi. When the fixation point is at the back the cone is empty. (2) "For a second, there may be the sheer empty transparency of stereoscopic vision —that glassy filling; but this doesn't last." (7) "The fixation point pulls the black back out of the cone." When the fixation point is in front the figure is (6) "a black jelly," (9) "just blackness," (10) "a black solid."

H (5) "I take it as solid, but everything seen is localized in the surface." (8) "I can't say that it is hollow or that it is filled."

The reports are very much like those of Series *d*. A sees sides but not surfaces, and the disc is glassy. Where the fixation point is on the background (S 3), she again sees the cone through the small circle as a hollow glass shell. The sides for Bi, while they are gray or black, are films and not surfaces. The sides for H are not so definite as before, but they are still gray, and mean a surface (S 5). When the figure is taken as a tube they are less fluffy and more glassy than when it is taken as a cone. The small circle is still definitely a gray surface, and in one case (9) H says that the large circle is more or less filled. The break in the side is filled by the background, that is, the cone does not exist at that point, for any *O*. Even where the figure is empty through the small circle (C3) A sees a content through the sides. In the other cases there is the same difference in content through the small circle and through the sides, and even though it is filmy, it is also glassy or "slick" (C 3,6). Bi finds a black content only when the fixation point is in the front circle. He still says that the point, when it is set back in the figure, pulls out the filling, though he now gets a glimpse of a glassy filling line like that of stereoscopic vision (C2). H's content is unchanged. He sees only surface, but he takes the figure as solid.

*Series f.* We now introduced, by suspension from the carrier, 6 barely perceptible flecks, made of beeswax and painted gray, into the cone in such a way that some were scattered inside the cone and some on the surfaces. The *O*s (A, Bi and H), again seated 4.5 m. from the opening in which the figure appeared, saw the figure against a black velvet background.

*Surfaces.* A (1) "The sides more or less follow these little dots which round out the boundary. Where the dots aren't around, the boundary is vague, and I can't tell where it is. It is vapory." (2) "Where the dots are near the circles, I can almost see the boundary. Where the dot comes into the boundary it is less transparent, though there isn't more stuff there."

Bi (1) "Black, but this black is not localized definitely on most of the circumference." (2) "Sometimes I think I see the outside of the cone as a little lighter than the inside."

H (1) "The side is neutral gray. It is translucent, and two little spots seem to lie in, to be a part of, a bounding surface." (2) "The side is very sharply limited, bulky but soft, and rather transparent and glassy. There is a small amount of gray there, so that it's a very weak positive experience of grayness." (3) "Though it is transparent, the side blots out, obscures the background." (1) "The front plane is slightly grayish, but not as gray

or as soft as the side; little more like a surface, though more transparent than the side." In only one case (3) H says: "no back or front plane."

*Contents.* A reports once (3) "Through the small circle the content is like the mist that's outside, but it's more dense, colorless and transparent. Through the sides, it's more misty, less transparent, and gray." Usually A says "a glassy liquid mass."

Bi (1) "The cone has substance which is more like the clear glassiness of the stereoscope. This jelly is not as sparkling and glittery as that in the stereoscope, but it's the same bulkiness." (2) "A black jelly (not a film black), or a clear thing that I see the black through." (3) "The cloudy black has come back again; clear up inside the cone. What I see in the small circle is much blacker than what I see as surface." (In this case the fixation point was in front of the figure.)

H sees only one figure (where there is but one dot in the cone), solid. (2) "Perceptually it is taken as a solid, but I really don't see anything inside certainly; a suggestion that the gray from the sides extends indefinitely inward, but I don't really see it." (1) "The flecks inside the figure make it more definitely empty."

For A the flecks seem to help in the formation of the sides and in their localization; while for Bi and H they do not affect the surfaces. For Bi the surfaces are of indistinct black, for H they are gray, and at the same time rather transparent and glassy. The front plane for H is different from the sides, in that it is "less gray, less soft, and more like a surface" (S 1). A and Bi definitely report content glassiness (C 1), and the presence of a glassy medium might also be inferred for H (C 2), since he describes the surfaces from which this gray extends as glassy (S 2). A and Bi agree upon the figures whose content they describe as glassy, and when the flecks are scattered in the fore part of the figure, both report a misty or cloudy filling, through for A this mist is colorless through the small circle, and gray through the sides, and for Bi it is a cloudy black. For Bi "the dots keep the black film out," and for H they make the figure "more definitely empty." A does not report any effect of the dots, but since her reports are very like Bi's, we may suppose that the effect was the same for her as for him. Even when H sees a solid figure, he says that this is perceptual, and that there is really nothing given. It is to be noted that the "glassy filling," which Bi now finds when the murky blackness is kept out of the figure, appeared fleetingly in Series *e*, Bi (C 3), under like conditions.

*Series g.* By turning *O*'s chair slightly to the side, and asking him to fixate a point placed directly in his line of regard and therefore to the side of the cone, we obtained (adding to the regular instruction: "Please fixate sharply the point which has been provided") a report of the cone as it appeared in indirect vision. The distance of the point from *O* varied, so that in some cases the cone was beyond the point of fixation, and in other cases the fixation point was beyond the cone. *O*s were Bi, By and Bs.

*Surfaces.* Bi does not see a figure. (1) "I would infer that there is a cone over there, but I don't see it. All I see are two rings with space between them."

By (3) "The thing does have sides because it's a cone, and it has sides to it as a cone should have, but when you try to describe them it hasn't any, but even then it still has the cone character. These sides are black." (2) "Black sides if you don't look for them."

Bs (1) "I can see only one side very well, and that looks quite gray. It is a haze or a cloud." (2) "The front hole is black, and it looks deep and bulky, like heavy black smoke, but it's really just around the top part. It doesn't fill the whole cone."

*Contents.* Although he does not get a figure, Bi says: (2) "They look like hoops hung in this stereoscopic jelly. This jelly is perfectly clear with

respect to the murkiness in it. I think it's black or else it's clear like water and I see the black behind it." (3) "I wouldn't attribute the black to the jelly. I think the jelly is colorless glassiness, and the black is behind."

By (1) "I don't know whether it's filled or empty. It may have a deep rich black inside, like the blackness in a tube, or there may be just a plane surface across the top. This surface is, in this case, black." (2) "The black of the contents, if it has any contents, is deeper and richer than the black of the sides."

Bs (1) "I can't say whether it's filled or empty." (2) "It seems filled sometimes with a blackness, but that gets away from me when I try to describe it and then it seems empty."

Bs sees the side surface as a gray cloud and the front surface as a black smoke. Bi does not see a figure at all, although he could infer a cone; and By sees black sides which will not bear inspection. Neither Bs nor By can say whether the figure is filled or empty. Both suggest a filling of 'blackness' which disappears for Bs when she tries to examine it, and which for By may be merely a black surface. If there is a content, both *O*s report it as blacker than the sides. Bi is the only *O* who reports glass. While he does not get a figure, he sees the circles hung in a 'stereoscopic jelly' (C 2). He is not sure whether the clear jelly itself is black or whether the black is behind it, but he is inclined to believe that the jelly is a colorless glass.

*Series h.* The *O*s (A, Bi and H) sat at a distance of 4.5 m. from the opening in which the figure appeared upon a black background. We used the figures of Series *a*, but altered the illumination, so that the figures were illuminated from different positions and with different lights. They were directly illuminated from the front, from below, and between the circles and at the front by the light from a 200 watt Mazda lamp, which was diffused by means of a single layer of white tissue paper; and they also appeared in daylight. Lighting from the back or from the side proved unsatisfactory, as then part of a ring or an entire ring was in too deep shadow for observation. In some cases we introduced a fixation point, as in Series *d*, and then added to the instructions "Please fixate the point which has been provided."

*Surfaces.* A (1) "Not really there though I see where they ought to be." (3) "If it had boundaries, it would be slick, I think."

Bi (1) "The sides are still there, though they're not so conspicuous as before." In only one case *O* reports (4) "a black shell."

H (1) "The sides are a neutral gray, very thin and transparent. Perceptually it is a surface, but not one that I could put my finger on. The side where I look is always grayer, but when I look to see if it's there, then it's indefinite." (2) "The little circle is glassy-like. It's like the sides, a transparent, grayish surface, yet not a surface." (4) "The side is a little loose, indefinite in localization, but taken as a figure it's definite enough."

*Contents.* A (1) "Through the sides the contents are filmy, but slick-looking. Through the small circle they are liquid looking with something slick about it." (3) "It may be colored but, if it is, it's perfectly transparent."

Bi (1) "A black filling which is soft, foggy, and bulky." (2) "More like black air than like black smoke." In case (6), where the fixation point is at the back, *O* reports that the figure is a "hollow shell" and adds: "By hollow, I just mean free from black. It is a clear filling."

H sees only those figures as solid which are without fixation points. The others are empty. (1) "I take it as solid, but this is pure meaning. I'm sure there is no real experience given that is localized in the figure. Anything approximately localized appears at the surface."

A returns her usual report. She sees where the sides ought to be, and adds that if they were there they would be 'slick'. Bi and H also describe sides, which for Bi are black and for H gray; but H says that the surface,

while definite enough for the figure, is not one "that I could put my finger on." H is the only *O* who describes a glassy surface (S 2), though A uses the term 'slick' (S 3). The content for A is again different through the smaller circle and through the sides. Through both the content is slick, but through the sides it is a slick film, and through the small circle a slick liquid. A cannot say definitely that the content is tinted, because it is transparent. Bi says that the content is a black filling, like black air; and again, where the point lies at the back, reports a clear figure, *i. e.*, a figure "free from black." For H the points empty the figures, and though where there are no points he takes the figure as solid, he says that the experience which he localizes is at the surface.

(2) *Experiments with Geometrical Skeleton Forms*

These figures, made of fine wire, were outlines of tridimensional geometrical forms: a cube, a rectangular, a rhomboidal, and a rhombic parallelopipedon; a pyramid with a triangular base; a pyramid with a square base; two pyramids with triangular bases fastened base to base; two pyramids with square bases fastened base to base; and two pyramids with triangular bases fastened apex to apex, to form an outline hourglass figure. The figures measured from 10 to 20 cm. on a side. The *O*s (A, Bi and H) sat at a distance of 2 m. from the opening behind which the figures appeared.

*Series a, b, c.* These series were repetitions of Series *a*, *b* and *h* of Part (1), *i. e.*, simple presentations of figures on black velvet background, on different backgrounds, and in different lighting.

*Series a. Surfaces.* A (2) "Light gray, but I can't tell whether I see them or the background through them." (3) "A sheet or a very fine conweb, which is soft and filmy looking; like glass, but softer than glass." (4) "Though I see the sides as planes, they don't look like surfaces."

Bi (1) "The forward sides never fill up, but the black fills in the faces seen through the other sides, as a window is filled with the black of night." (2) "Sometimes I get a shimmery something over the lines and filling the base. It's active and dancing, but not sparkling or clear." (4) "The other (front) sides are just open. They are nothing but rims around spaces which might be full of something."

H (2) "The sides are all filled; definitely gray and glassy-like." (3) "The top and bottom are somehow less definite than the sides." (4) "Casually the back appears filled; critically, I can't say. All the gray is in front of it."

*Contents.* A (1) "I am not sure about any of the contents. The figures seem solid, but I can't tell whether there's anything behind the bounding planes or not." (5) "Soft like a film, but there's more in it, and it's stable like jelly. At the same time it's a little glazed, like heat rising off a cornfield, except that it isn't as light."

Bi (5) "I can never fill the figure with anything."

H (4) "I take it to be solid, yet so far as actually seeing the solid, this is not true." (3) "Ordinarily I don't see anything inside the figure, yet the side doesn't leave off anywhere."

Both A and H see the sides as planes. H says they are gray, but A is doubtful whether they are themselves tinted, or are tinted by the background. Bi sees planes only in those sides which he sees through other sides, and then not as surfaces, but as filled with the film of the background. For A and H the figure seems solid, although neither *O* is satisfied that he actually sees content. The figure is empty for Bi. All of the *O*s report glass; A and H as surface and probably as content (if there is content), and Bi as surface over the base of the figure.

*Series b. Surfaces.* A (1) "The sides are made of glass but they're softer than glass." (6) "They may be gray, but I believe they are colorless and I see the background through them." (8) "This soft cobwebby stuff fills the planes; a film, not a surface."

Bi (2) "Taken naturally there are no bounding planes. The sides which are seen through other sides fill up with the background." (6) "It is possible to see the base filled with something shimmery like the glassy stuff, but it's quite transparent, like jelly or clear glass, or slightly smoked glass, because it looks blurry."

H (3) "The sides are gray; a transparent gray and not a hard definite surface." (5) "The top and bottom are softer, less dense, less gray, than the sides." (8) "I can't say about the back surfaces. The thing seems glassy, and I see the back through the other surfaces."

*Contents.* A (2) "I can't be sure about the filling. It may be or may not be filled." (4) "I seem to see something hazy as filling, as if you had something lively and stopped it before it had time to die." (5) "Almost like a block of ice, only it's not hard enough or thick enough to be an ice-filling. It still bothers me to call this content."

Bi (1) "The background really envelopes the whole figure. The figure is hung up in this thin smoke, so it's full of it, but it has no content as figure."

H (2) "I take it as a solid figure, which *is* filled, but I don't definitely see that stuff inside." (3) "It just gives the impression of glassy thickness."

The results are like those of Series *a*, except that Bi, with the gray background, sees the figure hanging in a thin smoke which fills it, but not as content of the figure.

*Series c. Surfaces.* A (1) "Like ice, but not so hard or thick as ice." (3) "I think that as an object the planes fill in, but I don't believe they do it so well otherwise." (5) "A congealed mist, which is stable as fine gelatine, but softer." (7) "First the figure was in outline, but when I found out what it was, I saw planes."

Bi (4) "The background fills the back faces, but the other faces are hollow." (7) "I can get a shimmery something like clear glass, or glass which *is* slightly smoked, but it's very light. It seems to fill the base."

H (1) "The sides are very gray. Some of the surfaces are more hard and glassy-like than others." (4) "The back surface is seen through the other surfaces and has a purely inferential grayness." For the complex figures *O* says: (7) "There is a dividing plane between the two figures which is glassier, denser, perhaps thinner in the sense of thickness, than the other sides. It is transparent. The sides are softer."

*Contents.* A (2) "I can't be sure that this is a solid figure. It seems solid all right, but if it were, I'd only be seeing it through itself." (5) "Like a block of very soft air, just so dense that I can see it." (6) "Glassy and congealed, as if the thing inside were 'caught in the going'."

Bi (1) "The figure is empty, but sometimes it's like a solid block of black." (5) "When it has a content, this content is really lighter than the background; really light gray, though I take it as black." (6) "Sometimes like a block of jelly, that doesn't shine like jelly; no highlights, and it has the effect of being transparent, though I don't see the background behind it."

H (3) "The figure is seen as solid. The inner surface or edge is very indefinite, but I don't really see anything inside." (5) "The sides are glassy, gray, and transparent, and they don't stop inside, so the content is like them, but I don't see it definitely as content." (7) "The gray I see belongs to the figure, it doesn't project beyond it."

These reports are in general like those of Series *a*. Bi, however, describes both an empty and a filled figure. The solid figure is black, and evidently not glassy. This report, since it was given with objects some of which were identical with those of Series *a*, probably indicates a change of attitude.

(3) *Experiments with Irregular Ridges*

We suspended two large sheets of gray cardboard 45 cm. apart in the carrier, in a vertical position, perpendicular to *O*'s line of regard. The upper edge of both, which had been cut into irregular ridges, was the only one visible to *O*. The ridges of the two sheets were asymmetrical; the front sheet hung 10 cm. below the back, which in turn hung 10 cm. from the top of the opening, and 45 cm. before a black velvet background. As *O* looked into the opening, he saw two sets of gray ridges, one higher than the other and at some distance behind it, and behind this, a black background.

*Series a.* This series was a repetition of (1), Series *a* and *h* combined, *i. e.*, simple presentation of the figures and presentation with different lightings. *O*s were A, Bi and H.

A always reports as in (3) "I simply see extended nothingness. I don't see it filled in at all."

Bi (1) "Between the ridges there is a haze. It's foggy and penetrable, a somewhat transparent film. It shades off and up, so that it's thinner at the top." The other reports are the same, except that the haze is lighter or darker with different intensities of light.

H (1) "The space is filled with gray, which runs in a band (but this band isn't a surface) between the irregularities of the gray mountains." This gray for H becomes lighter or darker with different intensities of light, but the report itself, except for this, is unchanged.

A change of light did not materially affect the filling in those cases where it was present. The haze of Bi and the gray of H simply become lighter or darker. A does not see a filling. The space is not described as glassy by any *O*.

*General Summary*

A, By and Bi always find indefinite sides, except for A (1) Series *f*, Bi (1) Series *c*, and all *O*s (2) Series *a*, *b*, *c* and (3) Series *a*. For H, who did not observe in Series *g* and *h*, the surfaces are always gray, and for Bs in the one series in which she observed, (1) Series *g*, they were gray. A sees plane surfaces in (1) Series *f*, and in (2) Series *a*, *b* and *c*. Bi sees definite surfaces in one series, (1) Series *c*, and occasionally also in (2) Series *a*, *b* and *c*. All of the *O*s see some of the figures as solid, though the content differs for different *O*s. A sees the content differently through the sides and through the small circle, and both A and H tend to become doubtful of the reality of a content. In (1) there are definite reports of glass both as surface and as content, and also cases where its presence may be inferred. In (2) and (3) no glass is reported.

## (*C*) *Experiments with Mirrored Forms*

The suggestion for this group of experiments came from a report in which *O* described what she saw in the skeleton figure as "like the mirrored reflection of black velvet which shows the glaze of the mirror and the velvet too."[24] We used (1) skeleton cones, (2) irregular ridges, and (3) simple objects.

(1) *Experiments with Skeleton Cones*

Two m. in front of the carrier in which the figure hung, we placed a 45 × 50 cm. plate glass French mirror. The reflection passed from this

[24]See p. 262.

mirror to another similar mirror of the same dimensions, which stood at an angle of 45° to the first, and into which the *Os* (A, Bi and H) looked. We presented series of skeleton cones corresponding to (*B*) (1) Series *a*, *d*, *f*, *h* (simple presentation, introduction of a fixation point, introduction of flecks, and different lighting).

*Series a. Surfaces.* A (1) "Both the little circle and the sides are misty, and the mist is glassy." (2) "Not much difference in the texture of the little circle and the sides, except that the sides are more like mist"; (5) "indefinite."

Bi (1) "Black; harder and solider, and not so soft and filmy as the unmirrored figures were; black but grayish." (3) "Though I say these surfaces are black, they are always slightly grayish, and they're never soft and fluffy as the unmirrored ones were."

H (3) "The sides are a transparent neutral gray; slightly fluffy." (5) "The near end is also a transparent gray; glassy-like, more solid, more bulky than the sides."

*Contents.* A (2) "Through the small circle, the contents are liquid looking, and through the sides they are misty." (3) "Looks liquid through the small circle; closer to a bulk than to a film; more like jelly than glass."

Bi (1) "Filled with black." (2) "The black filling is fluffy, but sometimes it changes, and the figure seems filled with black jelly."

H (1) "I take the thing to be more or less solid, but what I see is a completely bounded figure." (5) "I really don't see a content, though I take the figure as solid."

The reports of both A and H are like those in the stereoscopic and the real figure experiments. The boundaries for A are indefinite and misty, and for H are transparent gray. A does not find as much difference between the surface of the small circle and the sides as she did in the other experiments, but she notes the same difference in content through the circle and through the sides. H takes the figure as solid, though he sees only boundaries. The grayish black surfaces which Bi sees differ with respect to hardness from the unmirrored figures. The content for him is either a fluffy black, or a black jelly. A describes the mist as glassy (S 1) when it forms the indefinite boundary, and H (S 5) finds the smaller circle more glassy than the sides.

*Series b. Surfaces.* A (1) "The side is vague and misty. It is transparent, without any color. The little circle is definitely transparent like glass; definitely glassy, like isinglass." (3) "The sides are misty, but a bit glassy. The small circle is like isinglass, or like mist frozen into glass."

Bi (1) "The side surface is grayish and harder than in the unmirrored figures. It seems more solid and not so soft." (2) "When the fixation is at the back, it pulls the black out of the figure and leaves a gray surface shell."

H (2) "The sides are gray; not sharp surfaces, but the gray forms a sort of plane between the circles." (3) "The near end is gray, too, and is more glassy-like than the sides."

*Content.* A (1) "Liquid-looking through the small circle and bulky and misty through the sides."

Bi (2) "When the fixation is at the back, it pulls the black out of the figure." (3) "Black and fluffy, but still hard; sometimes like a black jelly."

H (3) "The figure looks solid, yet I can't say that I see any content. What I see is boundaries."

The fixation point does not affect the figure for A or for H. The smaller circle and the vague side are different for A both as to surface and as to the content seen through them. She reports a surface glass, which is apparently more definite in the smaller circle (S 1). The surfaces for H are gray; the familiar difficulty is present as to content; and the near end is said to be

"more glassy-like than the sides" (S 3). As in Series *a*, Bi finds the gray surface harder than that of the unmirrored figures, The fixation point, when it is not in front of the figure, pulls the black filling back with it.

*Series c. Surfaces.* A (1) "The sides are not definite, but where the flecks are there does seem to be a more definite boundary."

Bi (2) "Sometimes it seems as if there were a conical shell uniting the two circles, but I don't see it except as a lightish graylike stuff, which isn't really a surface."

H (1) "The side is a very thin veil of gray. It is almost transparent. The front plane is gray, and it is more transparent than the sides."

*Contents.* A (1) "Through the small circle more liquid than through the sides. Through the sides it is film-like." (2) "Some of the flecks are in this film-content. I see it as black content (through the sides) but I can't say whether it's black itself, or perfectly transparent, with the black behind it."

Bi (1) "A transparent jelly-like stuff; icy, airy; not black ice; but it is not quite clear. If I miss the flecks it tends to fill up with cloudiness." (2) "The black murky stuff does not come nearer than the back fleck. The figure seems carved out of glass."

H (2) "The flecks emphasize the emptiness; no hint of content, even in meaningful terms."

The reports are very much like those of (*B*) (1) Series *f*. The flecks help A to form bounding surfaces, but do not affect the surfaces for Bi, who finds them indefinite, "not really surfaces," or for H, who sees the gray planes. A sees a liquid content through the small circle, and a film-content through the sides. She cannot say whether it is colorless or black. The flecks for Bi keep back the cloudy black, and leave the cone filled with a transparent jelly. H again notes that the flecks empty the cone.

*Series d. Surfaces.* A (1) "The smaller circle is slick like glass, but the sides are more film-like." (4) "The front circle looks darker than the sides. All the boundaries are very indefinite though."

Bi (2) "Something like a gray shell there, but it's not a surface." (3) "The filling has no definite surface limit. The little circle seems darker than the rest of the figure."

H (1) "Both the side and the front planes are a neutral gray." (3) "The side is soft and fuzzy, and the front plane is more transparent, but they are both fairly definite."

*Contents.* A (2) "Through the small circle, positively transparent and glass-like with the shine and the slickness of glass; through the sides, lighter and filmy."

Bi (1) "A blue black, rather glassy, cloudy, murkiness; foggy." (3) "Sometimes the figure seems set in a glassy, rigid stuff."

H (2) "I do not see anything that I could call a content. I believe that the figure is hollow."

For A the indefinite surfaces and the content are different in the front circle and in the sides. The small circle is glassy and the content through it is glassy; the sides are filmy, and the content through them is filmy. There is a very positive report of glass, A (C 2). As opposed to the hard surface of Series *a*, Bi sees a gray shell which is not a real surface. The content for this *O*, while still black, is glassy, and his report under (C 3) is new. H sees gray planes for surfaces, but he does not report a content.

*General Summary*

The observations from experiment to experiment are very much alike. A finds indefinite surfaces and a content which is different through the sides and through the smaller circle. For H the sides are always transparent

and gray; and although he takes the figure as solid, except where flecks empty it (Series *c*), he is not sure that he sees a content. Although, in Series *a* and *b*, Bi says that the sides are harder than those of unmirrored figures, he decides in Series *c* and *d* that they are "indefinite, not really surfaces." The content is either a fluffy black or a black jelly, depending upon the position of fixation points or flecks (Series *c* and *d*). A and H both indicate surface glass. A reports a content glass, which is also present for H by implication (*i. e.*, like the sides), if there is a content.

(2) *Experiments with Irregular Ridges*

We suspended the irregular cardboard ridges of (*B*) (3) in the carrier and mirrored them as in (1) Skeleton Figures. We changed the lighting during the series, following (*B*) (3).

A (1) "There must be something between that holds these things apart, but I don't see it."

Bi (1) "The filling between them has no definite surface limits. It's a yellowish haze that's hard and glittery; transparent, but almost opaque." The only difference which *O* reports with change of light is one of tint. As the light was moved farther forward, the haze became grayer.

H (2) "The space between, if I fixate it, becomes a solid roof; light gray and soft. It means solid. It is not transparent. If I fixate the top of the figure, the space between is seen as slightly blurred, hazy, and grayish, like the stuff one gets on a hazy day."

A does not see a filling, but Bi and H do. This filling is for both *Os* a haze, which for H is gray and for Bi yellow. It is not transparent for H; for Bi, though it is transparent, it is "almost opaque." It is soft for H and hard for Bi, who adds further that it is glittery. For both *Os* tint alone is a function of illumination.

(3) *Simple Objects*

We placed 3 simple objects (a 7 × 12 × 17 cm. box, a 3 × 10 × 35 cm. and a 3 × 12 × 60 cm. smooth board) on a table before one of the mirrors used in (*C*) (1), in such a way that in the reflection a portion of the table surface appeared between the objects. This visible surface appeared first equal in extent between the objects, then greater between the first and the second objects, then greater between the second and the third objects. The *Os* (Bi, By and Bs) who sat 3 m. from the mirror, could see the objects but not themselves in the mirror. We obtained a low degree of illumination for the objects and the mirror by passing the light from a 200 watt Mazda bulb through a heavy tissue paper screen. The bulb and the screen stood to the right of the objects, and were cut off from the *O* by a black velvet curtain. The instruction read "You are to describe what you see, noting particularly the space between the objects."

Bi (1) "I'm looking for that jelly-like stuff but I don't see it. There is nothing but the table between the objects. The third dimension comes in somehow, but it doesn't seem to be anything visible. It's more like knowledge about space than anything visible."

By (3) "The only thing I see between the objects is the brown quality of the table. There is a suggestion of a white colorless filling, too, but I'm not sure that I really see it."

Bs (3) "That space is filled all right, but it's absolutely clear and transparent. It seems to be a thick air, like that you see on a heavy rainy day, just before it begins to rain. It reminds me of the dust particles in a sunbeam, though here the particles are quite close together. It looks glassy, too, like looking into a paper-weight."

Bs is the only *O* who reports a positive filling. She notes the presence of fine particles and says that the filling is glassy. Both Bi and By see only the table between the objects, although By finds a suggestion of a white colorless filling, which he is not sure that he really sees. Bi prefers to ascribe his realization of the third dimension to what he knows rather than to what he sees.

## (*D*) *Miscellaneous Experiments*

### (1) *Experiments with Wall-Papers*

Both Brewster[25] and Helmholtz[26] have observed that by squinting the pattern of a flowered or figured wall paper may be brought close to *O*. We performed our experiments of this type in the darkroom, where we suspended 1.5 m. before the *O*s (Bi, By and N)[27] 2-meter squares of cretonne, linoleum, and wall paper, in which a pronounced pattern (about 25 cm. square) was repeated at regular intervals. For illumination we used a 200 watt Mazda lamp, the light of which was filtered through a Gage 'daylight' glass, faced with a double screen of tissue paper. The instruction read: "By squinting, you are to lift the pattern from the background. Then you are to describe what you see, noting particularly the presence of any glass in the experience." After all the experimenting that we had done,—(*D*) followed (*A*), (*B*), and (*C*),—we felt that we should not influence the result by asking directly for glass. Since *O* found it difficult, at first, to converge in front of the pattern, we gave him a steel needle upon which he fixated, as he held it out before him, and which he discarded as soon as he could proceed without it.

Bi (1) "All the patterns are set in a glass which covers the whole field. I don't believe that I'd say that the patterns themselves are glassy." (3) "I get glassiness all right. The patterns are set in it, but it's so out of focus that I can't tell about the distance between figure and background. It's not as flat as paper normally is though." (4) "There is the impression that these things are set in glass, though it's out of focus and I see it poorly." (7) "This thing is filagreed and set in something similar to glassiness. It's bulky like a glass, but it has little particles dancing in it."

By (2) "I think I do see something there, but it's hard to say, because the background looks filmy. You seem to see something like what you see in the stereoscope, but it doesn't have quite as much body as the stereoscope stuff has. I think it's there and surrounds the whole flower in a thin film." (3) "I certainly see a lot of glassy stuff everywhere. The pattern is off the background, and the glassy stuff is between it and the background, and over the pattern and between me and the paper. It seems filled with it." (5) "The glassy stuff is there in a big cloud, only cloud isn't a good word. It is denser and has more body than a cloud has. It is very smooth, and it glitters. It looks something like jello, that is, it's not quite colorless, but has a tinge of the background color in it."

N (1) "Between me and the pattern there is a suggestion of what you see in the summer heat shimmers. Sometimes this glassiness is a thick cloud and sometimes it is a thin sheet before the paper." (4) "The glassiness is very solid and apparent, but the pattern does not stand out clearly in it. It is bleary." (5) "This stuff is in the nature of a thickening of some kind. It can be more or less glassy, and more or less thick, and it is very transparent. Glass is the best word I can think of."

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[25] D. Brewster, On the Knowledge of Distance given by Binocular Vision, *Phil. Mag.*, 3rd. Series, 30, 1847, 305f.

[26] H. v. Helmholtz, *op. cit.*, 798 f.

[27] Dr. J. P. Nafe (N) was a member of the department of psychology.

Bi and By are more successful than N in raising the pattern from the background, but all report the presence of glass which covers the whole field, and in which, for the first two at least, the pattern is set. Bi mentions dancing particles in it; By says that it glitters and has more body than a cloud, and N suggests the heat shimmer, either in a thick cloud or in a thin sheet. We may thus infer that By and N also saw something resembling a flecky medium.

(2) *Experiments with Funnels, Cylinder, and Pierced Card-board*

We next repeated some experiments described by Rubin.[28] Because our time was limited, we divided the experiments: By observed the funnels and the cylinder, and Bi and N the opening in the card-board. In order to discover whether or not there was a limit to the extent of the air-figure, we introduced a slight variation by using for By a series of 7 funnels, the openings of which ranged in diameter from 3 to 15 cm. by 2 cm. intervals. The instruction read "You are to describe what you see (*a*) while attending to and converging upon the distant object; (*b*) while attending to the irregular opening and converging upon the distant object; and (*c*) while attending to and converging upon the irregular opening. Note particularly the presence of any glass in the experience."[29] After the report on this instruction we asked By to repeat his observations with the same instruction, to which we now added "Move the funnel about over the field." We next requested *O* to fixate a point inside the funnel, and for some observations inserted a pin in the body of the funnel as a fixation point.

The instruction for Bi and N read "Fixate the edge of the hole in the card. Describe what you see *in the plane of the opening*, noting particularly the presence of any glass in the experience." We also asked the *O*s to move the card about over the field.

By. Under (*a*) of the instruction, *O* always reports: (2) "I don't think there's any glass there. It's hard to say, because it's hard to follow this instruction, but I think that I am following (*b*) when I get any glass." Under (*b*); (1) "There is a thin film of glass over the end of the tube. It is like a little window, and that film is very thin. The object at which I am looking is a thing by itself." (2) "The film moves with the funnel, as if it belonged to it." Under (*c*); (2) "The space beyond the tube is filled with a whole bulk of that glassy stuff. The object beyond it becomes indistinct and like a film." (5) "When I move it the glass bulk doesn't move." The reports were similar for all the funnels except the largest (15 cm.). Here the glass disappeared, both as film and as bulk. With the introduction of fixation inside the funnel; (1) "The hole comes up to where I fixate. The tube is foreshortened. The glass is a bright, brilliant sheet across this opening, but it doesn't come into the tube. The tube is just foreshortened." (2) "When I use the pin and fixate on it, it appears to lie in the hole. The glass is very much present. The pin lies in it. I think it is a bulk, not a sheet. If I move the pin it moves around in the glass. I don't think it comes up into the tube."

The reports for the cylinder were like those for the funnel.

Bi (1) "There is a filling that does not come this side of the hole. It's like the clouding that you get in water by stirring up a light sediment that's gone to the bottom. But this doesn't have a front surface." (4) "This is excellent glass. The rough places in the background look as if they were covered up with a great mass of glass, which fills everything solidly from the hole backwards. The glass is so real that the cardboard seems to lean

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[28] E. Rubin, *op. cit.*, 61-66.

[29] That this instruction proved too difficult for him to follow is evident in *O*'s report.

up against it. It's perfectly clear, but it has streaks and splotches in it that don't seem to belong to the background. It doesn't seem to have a front surface."

N (1) "I see a solid glass film lying just behind the opening. It's of indefinite depth, but there's more of it than a plane; like looking into a block of glass, except that there are no imperfections in it. The lines of the background seem to lie behind it and they are blurred."

Later, both *Os* said that they saw the thin film in the plane of the card.

By finds not alone the thin glass film of Rubin, but a bulky mass beyond the funnel, or no glass at all, depending on instruction. Neither the film nor the bulk appeared for him with the funnel in which the irregular opening was 15 cm. in diameter. Bi and N practically duplicated with the cardboard the results of By with the funnel. They found a bulky glass beyond the hole and a glass film across it.

(3) *Experiments of Jaensch with Colored Liquids*

We also repeated the experiments described by Jaensch with flasks and threads.[36] We used a red and a yellow liquid. *Os* were Bi, By and N.

Bi (1) "The glass is there in the figure. I get a murkiness that is yellow and that is associated with the glass like a dye or stain. I think the glass is colored as a paper weight is colored. It's a solid yellow prism all right." (3) "Now the prism is perfectly clear, and the color is back. That clearness is glassiness though, and it looks solid and hard, without quality in the ordinary sense. I mean it's neither gray nor color, film nor surface, but it has a transparent glittery character." (4) "I get planes all right, but I can't see any surfaces."

By (1) "A yellow prism. I can't tell much about it because it is set in a mass of stuff just like itself. This is glassy, thick and very dense." (3) "A glass prism. It's difficult to say what the sides are made of. It seems to be set in a block of glassy stuff which is colorless, shiny, lustrous; and because it is made of this stuff too, I can't say much about it. The yellow lies behind the figure somewhere; at least the figure itself is colorless."

N (1) "A yellowish-brown prism. It is transparent or at least translucent. I really don't see any planes, I just read them in, I think." (3) "I take it as a solid figure; transparent and made of glass. I think I see planes, but it is not so easy to be sure of them, because, since it is transparent, I don't localize anything inside." (5) "It seems to be a luminous figure, whether I see it as colored or not."

Our results agree with those of Jaensch. Whether the figure is colored or not, the *Os* describe it as glassy. While they take it as a solid figure, they cannot say much about either surfaces or content, except that what is there is glassy.

(4) *Experiments with Field Glasses*

We asked H and By to observe bits of landscape through a pair of field glasses. The instruction read "You are to describe what you see, noting particularly the space between things. Please note also the presence of any glass in the experience."

By (1) "If I don't look at any particular spot, the whole field seems set in a block of glassy stuff. If I look at some special spot, as between the twigs, then I see a thin sheet of something like clear glass between them." (3) "I am sure now. In a small space, like that between two branches or twigs, there is a thin lustrous glass sheet, but when I take the whole field, then there is a bulk of glass all over it."

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[36]E. Jaensch, *op. cit.*, 266ff.

H (1) "If I look at the space between the limbs, and if the limbs are fairly close together, there is a blurred effect that looks like a slightly lighter, grayish, very dense, highly translucent mass, that nevertheless is a little soft. If I look away at the side of the hill below the house, there is still something like a blurring effect, but it is a little lighter and more transparent. It seems to be more of a general mass without any boundaries at all. I don't see any particular space that way, but just a total space out there." (2) "Where the limbs are close together, I get a massive blurred effect. This is apparently something seen; something light gray, slightly lighter than the building which I see between the branches. It is localized in an indefinite mass all around the tree. If I focus for the tree in a place where the branches aren't so thick, the spaces seem to break up into individual areas. Then I get something like a surface-filling filling up that particular space. This surface seems to start from a slight chromatic effect, but I can't say that I really see anything actually between the things except these slight starts from each side." (3) "I get images of the blood capillaries in the eye. If I look closely, there is an effect of waving, quivering, and it gets localized at the plane between the bounding branches. It seems to generate a plane."

The *O*s agree that there is a filling which is a kind of bulk and which fills rather large areas, and a surface-filling which appears between the boundaries of small areas. H says that both types of filling are gray and blurred, while By describes them as "glassy."

(5) *Comparison of All the 'Glasses'*

At the conclusion of our experiments upon the "glassy sensation," we gave the *O*s (Bi and By) a figure from each group—(*a*) a stereogram, (*b*) a wire figure, (*c*) a colored liquid in which 3 threads hung, and (*d*) a square of wall-paper—and asked them to compare the glasses which they found in them.

Bi "They're all alike in their bulky tridimensional character. Sometimes the glass is perfectly clear and sometimes it isn't. It all glitters, though some doesn't glitter quite so much as others. It blurs too, more or less, and some is harder than others. It gets smoky or cloudy, too, but it's all the same kind of stuff."

By "The glassy stuff is the same kind of stuff for all of them. It may be more streaked in some cases, or it may be of different thicknesses, but it's the same kind of material in general. I think they're all different steps in the same series."

The *O*s agree that the glass in all the experiments is fundamentally the same kind of thing. The difference is not one of nature but one of degree.

## (*E*) *Casual Instances*[31]

(M) "This morning while N and I were talking during an interval in his experiment, I suddenly noticed that the space between the two of us was filled with glass. N seemed to be set in a heavy transparent stuff that was perfectly clear, yet somehow visible. It most resembled the heavy glass of a thick paper-weight, and it seemed oppressive, as if I were trying to breathe in spite of a crushing weight. This was at 11 a.m. The sun was shining brightly, but it had not as yet come into the window of that room."

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[31]T is Professor E. B. Titchener; Hu and K were students in psychology, but not trained *O*s; Bm and L were students taking their first course in psychology; Ma was an untrained *O*, interested in psychology; C and Hw were totally untrained *O*s, who knew nothing about scientific psychology. Some casual instances are also furnished by the writer, M.

(Bi) "I was looking at that tripod downstairs. It reminded me of your wire triangles, and I looked to see if there were any glass there. Then I saw it just the way I saw those figures, with the planes filled in with glass. It was a cloudy day."

(N) "This morning, just as the sun came around to my side of the building, but before it was shining directly into my window, I saw a lot of glass. The smoke from my cigarette curled up very slowly, in fact it hardly seemed to move at all. I saw it in a block of glass that extended out all around it. It looked hard. It was something like the thing you see in a sunbeam when the dust is dancing in it."

(M) "As I was coming across the campus the other night, I glanced up at the treetops and saw the branches held rigidly apart by some solid medium, which was perfectly transparent, and yet plainly there before the gray of the sky. It seemed to hold the branches stiffly, as if they were frozen in it. The night was a clear and rather light one in early fall, before the leaves had fallen. The glass first appeared when I was about 40 ft. away and stayed as I approached the trees."

(M) "I was looking down upon Baker Tower through an open window in Morrill Hall, about 4 p. m. on a rather cloudy day. I saw the Tower and all the trees around it set in a mass of glass. The glass lay heavily, it seemed, upon the roof. It was most dense and thick and had that solid transparency about it that a thick window-pane has. I saw it both binocularly and monocularly. It was at least 200 yds. from the window to the Tower. I couldn't say where the glass began, at all, but it was just there, everywhere I looked, about the way water looks when you open your eyes in it."

(Hu) "At 10 a. m. when I was coming toward the Library, the snow was falling very slowly. I saw the library and the snow as if they were all set in a glass paper-weight. I almost expected to walk into a wall of glass, only the wall was all around me, too. I was about 100 yds. away, I think, when I first noticed it."

(M) "Some students asked me to tell them something about the glassy sensation. As I started to talk, I looked at the door and saw it filled with a shin sheet of glassy stuff. As I mentioned the bulky character which it tometimes has, I suddenly saw that the glass in the door was no longer a thin sheet, but was now merely part of a bulk that extended into the next room as far as I could see. This was about 3 p.m. on a cloudy afternoon in March."

(M) "I was observing for S in the dark-room. Everywhere between my eyes and the card which he was exposing, I saw a fine haze. It was transparent, yet filled with something like tiny bits of dust, and I want to say it was gray, though I don't think there was really any quality there. There was a rigid frozen stiffness about it. It interfered with my observation, and persisted during several exposures."

(By) "Seated in an automobile, I saw the space between the body and the top filled with glass. A cigarette-end held outside the car looked like the reflection of a cigarette-end in a mica mirror. There was an electric street-lamp directly above the car. The glass was slightly yellowish; it seemed thick and dense. It looked less like glass than like isinglass."

(M) "We were driving at night along a road bordered on one side by fields and infrequent trees, on the other by a rather thick pine growth. There was no wind and the heavy snow flakes fell slowly with no decided directional trend. The whole landscape seemed unreal, as if the trees were made of stiff paper encased in glass. The snow fell in it, too, and the glass was still there, though one wouldn't have expected it to persist. The glass was there, whether one looked monocularly or binocularly, through the glass of the windshield, or out of the open side of the car."

(By) "I was sitting about 10 ft. away from the window, about 4:45 p.m. on a cloudy December afternoon. The electric light was on. The window, which is set in a kind of recess, seemed behind a box of bulky glass stuff. The box was formed by the recess. I couldn't see the glassy stuff if I looked at the edge of the box, but if I let the boundaries appear in indirect vision, the whole thing was filled."

(K) "Looking out of the window of the Laboratory in January, between the branches of a tree and the ground I saw a mass of clear, transparent, glassy substance, smooth, hard on the surface, the substance softer, more jelly-like. The mass had no definite boundaries."

(M) "I stood on the porch at night, looking over the city to South Hill. It was clear, but there was no moon. The whole valley seemed covered with glass, as some of the rock formations of Luray Caverns are immersed in water. The street lights twinkled, but I saw them through a transparent bulk. I shut one eye and looked and the impression persisted."

(C) C and the writer were canoeing on an afternoon in July. The sun was shining brightly. There was no wind. C suddenly said "Look at those trees across there (about 50 yds. away); they look as if they were painted on stiff air. Can't you see something between them that is hard and still? I suppose I am imagining it, but it looks that way."

(By) "At the end of the lecture desk in Goldwin Smith Hall there is a faucet attached to a right-angled pipe, which rises about a foot above the surface of the desk. I saw the space which was bounded on two sides by the pipe and on the third side by the desk as a square of surfacy colorless glass. It was highly reflecting and shiny. This was at 11 a.m. on a dull day in January."

(Ma) "I saw that glass this morning for the first time. I was walking up toward the Library, and I saw the building itself and the snow was falling around it as if they were in an aquarium. The stationary objects stood in something like water and the flakes fell the way food particles float to the bottom in fish-bowls. The thing that was there wasn't water, really, but that is the best way I can describe it."

(Bm) "I have been wondering what that 'glass' was that you talked about. Now I see it. It's between every snow-flake as if it fastened them together. It doesn't move, but it's always there between the flakes, and it doesn't stop them from falling."

(L) "I had heard of the glassy substance in lecture, but I doubted its existence. While sitting in a class and not even thinking of psychology, I looked toward Baker Laboratory and I saw that the atmosphere seemed to be entirely covered with this glassy substance. It was such a surprise to me that I showed it to the student at my side. It was as vivid as can be imagined and it immediately aroused my attention. I turned away, then looked again to make sure that I was not mistaken. It remained there before me. This was about 8:30 a.m. on an ordinary winter morning."

(By) "As I entered my room the other night the electric heater was on, but the light was not. Between the wires across the front of the heater I saw a surface of glass. The space behind the glass seemed filled with a red film. I could not say whether the glass itself was red or whether I was looking through a colorless glass at a red quality beyond it."

(Hw) Hw and the writer were walking along the road on a bright moonlight night in August. Hw said "I almost believe that I can see air. Doesn't it look as if there were a dancing, soft stuff everywhere? Of course I don't really see it, but I think I do. I can't quite describe it." The writer (M) noticed at the time that there was a bulky soft glass that seemed to be all around her.

(M) "While looking absently at a small table, it occurred to me to try to surround it with glass. I looked and saw that the table was set in a bulky glassiness, which had no very definite boundaries, but which was most in-

sistent between the table-legs. It was somehow more than a surface. Then I tried to destroy the glass, but the impression persisted. This was in a small room, under artificial illumination."

(T) "I was looking across the road at a slope of smooth snow running up the hill between fairly even lines of trees. The snow had become gray, and I absent-mindedly took it for an expanse of gray sky seen between the trees. Suddenly I noticed that the gray sky was glassy all through; and only then did it occur to me that the sky itself was a 'memory color' and that I was really looking at the solid slope of snow."

Both By and M find that, although they may call up the glass practically at will, once it has appeared they cannot destroy it.

*Conclusions*

The conditions which Schumann finds for the arousal of the 'glassy sensation' are (1) plasticity, which is aided by eye-movement; and (2) the localization of small points in the field, which may be a further aid in bringing about eye-movement with roving attention.[32] Although the presence of contours may attract attention, there are also cases in which the medium is present, but has no limiting surfaces. A secondary condition is the degree of compactness of the filling; which means merely that, where the medium is less compact, it is more difficult to observe.

These were, no doubt, the conditions to which Schumann was led by his observations. Our own far larger material, however, proves them to be inadequate. Thus (1) plasticity, in the sense of perceived tridimensionality, may occur in the absence of the 'glassy sensation'. If, however, the 'glassy sensation' appears, it is itself constitutive of plasticity. It does not seem, then, that plasticity can be set down as a condition of the 'glassy sensation'. Nor can (2) eye-movement be accepted without reservation as a condition of the phenomenon; for the elimination of eye-movement, as in certain of our experiments with the stereoscopic slides, does not sensibly change the reports upon the glass. It is, however, true that in some instances the point at which fixation occurs determines both the presence and the nature of the glass filling. We cannot grant either (3) that the presence of perceived flecks is a condition for the observation of glass, although our results indicate that the nature of the filling is influenced by their presence. As for (4) roving attention,—where, as in the Rubin experiments, we required a fixed direction of attention, we still obtained reports of glass, the nature of which again differed with different points of fixation. With regard (5) to compactness, our reports show that there are indeed different degrees of compactness of the filling, and that the more compact the filling, the more definite is the report of it. But this condition seems akin to that of plasticity, in that it presupposes the existence of the phenomenon under investigation.

[32] F. Schumann, *op. cit.*, 238ff.

Schumann makes no reference to object-consciousness. Indeed, his statement that, although contours attract attention, there are cases in which the glass has no limiting surfaces might be interpreted to mean that object-consciousness is not a necessary condition of the phenomenon. In our own experiments, however, the glass, whatever its extent over the visual field, was always localized. This rule, to which there is not a single exception, indicates that we are dealing with a phenomenon at the level of perception,—a conclusion which is borne out by the rôle which certain of our experiments ascribe to fixation. For the rest, we can ourselves give no conditions for the glassy phenomenon except the conditions for spatial perception at large. Wherever there can be spatial perception, wherever there can be visual object-consciousness by way of localization, there may also be glass.

The casual instances alone prove that it may appear outdoors or indoors, in summer or in winter, during the day or at night, on bright or on dull days, surrounding large or small objects, large or small in extent, near *O* or far away, monocularly or binocularly, to the naked eye or through lenses, under artificial or natural illumination, immediately or after an interval, with or without expectation, to trained or untrained *O*s. We have been wholly baffled in the attempt to put our finger upon some precise moment or moments that should control the appearance of the phenomenon.

We remarked at the beginning of this paper that we believe the 'glassy sensation' to be essentially imaginal in nature. During the experiments we were constantly meeting the fact that any change in conditions which brought about a change in the reports did not effect the same change for all the *O*s. This result leads us to believe that the phenomenon is largely of central, not of peripheral origin.

We are not, therefore, prepared with Schumann to admit a 'new sensation'. The very fact that the term 'sensation' is classificatory only, demands that the basis of classification be adhered to. We thus expect that the proposed 'sensation' shall arise as the result of adequate external stimulation, and that it shall have the attributes at least of quality and intensity.[33] But Schumann admits that, if such criteria are insisted upon, the 'glassy sensation' does not exist; for it lacks the attribute of intensity, and it has no adequate external stimulus. That it has the attribute of extensity (since it exists both as surface and as bulk), and that it may have different degrees of saturation are, along with quality, the grounds upon which he bases his

[33]Cf. G. E. Müller, *Abriss der Psychologie*, 1924, 76. "This (Schumann's) view, which would have to start out from the assumption of a spatiality of the original visual sensations, would become very important if it could be shown that to the impression of greater depth there always corresponds a greater thickness of that spatial sensation." There is no hint, in our observations, of any such thickening with distance.

conclusion that he has found a new sensation.[34] It seems useless on such grounds as these to attempt controversy. To accept the glassy impression as a sensation is, at any rate, to erect a new definition of that protean term.

We conclude, accordingly, that the 'glassy impression' is not a sensation, but a perceptive content which is largely imaginal. It may be that this content is to be regarded with Jaensch as identical with or based upon G. E. Müller's "central gray." It may be that it is based upon a projection of the humor of the eye, such as may be seen in the observation of *muscae volitantes*.[35] Or both of these factors may be concerned,—or other factors, which as yet escape identification. We hope that future experiments may decide this still outstanding question.

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[34]F. Schumann, *op. cit.*, 235 ff.

[35]This explanation was also suggested by Professor K. M. Dallenbach. Our observer H suggested that the projection of the retinal capillaries might afford the necessary basis for the phenomenon. Bühler's view seems to be negatived by the results of D. Katz' experiments (Neue Beiträge zu den Erscheinungsweisen der Farben; Luftlicht und Beleuchtungseindruck, *Zts. f. Psych.*, 95, 1924, 129-136). We have repeated and confirmed Katz' observations.

# GENERAL ANTHROPONOMY AND ITS SYSTEMATIC PROBLEMS

By Walter S. Hunter, University of Kansas

## Prospect

In the present paper we shall seek to justify the use of a new name for the scientific study of human nature. The necessity for such a change arises partly from the serious inappropriateness of the term 'psychology' as a designation for the science and partly from the discredit cast upon the field by charlatans and spirit-seekers. The discussion will also lead us to attempt to dispel the notion that a non-psychic study of human nature is mere biology.

## Argument

Anthroponomy is the science of the laws which govern human action—the science of human nature. It seeks by experiment and systematic observation to arrive at an understanding of the factors which determine certain of the observable phenomena of the human individual. This science of anthroponomy is a newcomer, having arisen gradually in opposition to its nearby relative, psychology. The essential difference between psychology and anthroponomy lies in the attitude towards the observations which are made. The former science is derived directly from the Greek philosophers and is concerned only with the study of mind, consciousness, or the psyche. This object of study is held to be directly accessible only to the subject who possesses it, and it is also regarded as a separate and distinct aspect of the universe. Such a point of view results, as might be expected, in the neglect of all data concerning the human individual which cannot be interpreted as evidence for the existence of some type of psychic process. In other words, scientific observations are not to be valued for their own sake, but solely because of the inferences concerning consciousness which they make possible. How different is the case of the anthroponomist! He never raises the question whether or not the observable phenomena express or embody a psychic world beyond them. He studies the large field of observable human nature in order to describe and explain the phenomena there found. When the question of the control of human nature arises, the anthroponomist proceeds in terms of the laws derived from his matter-of-fact study. Scientific psychologists are one with the anthroponomist here, inasmuch as matters of control are practical affairs and do not rest upon philosophical theory. However, a method equally valid theoretically is fol-

lowed by the host of unscientific psychologists, the method of psychic control which seeks to shape human events through spiritual influences. Although this effort and this school of psychologists are pooh-poohed by scientific men, the amount of unscientific work does not seem to abate. In attempting to avoid the problem of spirit-control and psychic causality, scientific students who still insist that they are investigating "consciousness" turn to the nervous system as an explanatory rubric. This general problem of explanation, as it arises in anthroponomy, will be discussed later (p. 300). At the present time, however, it should be pointed out that recourse to neural processes in psychological study leads the psychologist outside the psychic and involves him inevitably in philosophical theories of psychophysical relationships. Not only does this difficulty occur, but another equally serious appears in the uncertainty over the boundaries of his science. The human individual is so thoroughly an organism that it becomes a practical impossibility to admit one segment of the organism (the neural basis of consciousness) into a science and to exclude all other phases of that segment and all other segments. The neural processes accompanying the psyche are so organically related to other biological (neural and nonneural) processes that once the psychologist opens the door to the former he cannot perforce close it to the latter. The psychologist is thus led to expend much energy in attempts at the proper delimitation of his field. The psyche he cannot abandon and remain psychologist. If he includes in his science certain neural processes and stimulus-response situations upon the pretext of elucidating the psyche, he disrupts the organic character of the individual studied. If he reaches out still further and includes all of the data on behavior in which he may be interested he must defend himself from the implication that he has become a biologist.

It has been said above that the essential difference between psychology and anthroponomy lies in the attitude adopted toward the data gathered. One difference in this attitude is shown in the unwillingness of the psychologist to describe and explain observable data as such irrespective of their supposed relationships to a psychic world. The second difference in attitude arises from the first. Anthroponomy is essentially genetic and seeks to explain human nature from the simple to the more complex processes. Psychology on the other hand is essentially nongenetic and seeks to interpret human nature from the vantage-point of the most complex process, the assumed psyche. Anthroponomy begins with the relatively simple stimulus-response situations in man and animals and attempts to understand the more complex forms of behavior upon this basis. It studies peoples of varying degrees of cultural attainment, not in an at-

tempt to read psychic powers into them, but in order to understand the observable phenomena of the primitive peoples themselves. Knowledge so gained, it is believed, should clarify much that is found in the more developed groups. Animals are studied in terms of their own behavior-history as subjects interesting in themselves, but with the added conviction that the understanding of human nature will be advanced when the problems concerning infra-human animals are solved. Anthroponomists do not attempt to discover a psyche or a mental life in animals, nor do they treat the animal's responses as instances of introspection. Psychology does all of the things which we have said that anthroponomy does not do. It endeavors continually to find traces of 'psychic processes' in lower animals and peoples. Its experimental data on laboratory humans are almost entirely in the form of language-responses understood as symbols of the 'psychic'. *These responses and the problems arising from the analysis of their symbolism are taken as the key to the understanding of human nature, in spite of the fact that language-responses and the psychic processes symbolized are most complex affairs which are probably late in appearance in racial history.* This anti-genetic method is well illustrated in the treatment of instinct, where psychology seeks for conscious correlates and analogies in place of devoting itself to the description and analysis of observable unlearned behavior and its language connections, even to the extent of asserting that instincts arise by a process of lapsed intelligence. The nearest that psychology has come to a scientific genetic account of phenomena is in its attempts to show the origin of adult states of consciousness through analysis by introspection. Such studies, conceived in the manner of Wundt, Stout, and Titchener, however, are more aptly termed analytic than genetic. The reason for the essentially non-genetic or anti-genetic character of psychology lies in the fact that by definition the subject-matter and goal of the discipline are kept within the confines of language-responses treated as symbolic of the mental world. The psychologist cannot by definition remain a psychologist and go outside of these responses. And yet he must go outside and relate them to other and simpler stimulus-response connections, if their explanation is to be found.

Anthroponomy is in the process of being born. Much good work has been done in it even by the psychologist, for he has taken the chief interest in a scientific approach to human nature in spite of the fact that he has usually been diverted from the main topic by the will-o'-the-wisp of consciousness. Indeed the relationship between anthroponomy and psychology is not unlike that between alchemy and chemistry and between astrology and astronomy. The alchemists were the chief group of scientists interested in the different kinds of matter and in their

interactions. They were, however, obsessed by one problem, that of the transmutation of substances. It is unwise to call this either an insoluble or an insignificant problem. It is, nevertheless, but one problem in a large field, and it was insoluble for the men who approached it in the days of alchemy. These early students lacked the objective non-magical point of view of the present chemist. They lacked the great body of chemical knowledge and the refinement of technique necessary to attack the problem with a hope of success. And yet, in the pursuit of their goal, they made many solid contributions to knowledge which were incorporated into the later science, renamed chemistry. The parallelism is not quite so close with astrology and astronomy, inasmuch as the astrologers were chiefly concerned with the horoscopic aspect of astral observations, a problem which astronomy has repudiated perhaps for all time. And yet the astrologers made many careful observations of great scientific value. The casting out of devils was the great need in alchemy and astrology and is now the great need in psychology. With the attainment of an objective point of view, the magical mind-body problem, which is characteristic of the three pseudo-sciences of alchemy, astrology and psychology disappears. This prerequisite to complete scientific adequacy comes slowly but surely. But once the point of view is secured, a proper perspective of the task in hand is possible and a common-sense programme can be formulated. Problems can then be attacked in the order of their probability of solution under the conviction that only upon a genetic basis can the most complex and baffling phenomena be analyzed.

The connotations of the terms alchemy and astrology were so embarrassing to the scientific men concerned and so inadequate to describe the nature of the problems investigated that the new terms chemistry and astronomy have come into general use. Alchemy persists only in historical and literary usage, but astrology is still practised by charlatans and self-styled psychologists of a non-scientific variety. Psychology, like alchemy and astrology, is a term which is quite inadequate to describe the investigations of the scientific men to which it is applied. Inasmuch, however, as there is now a large body of general scientific information and a close communion between scientific men, the situation might be left to correct itself through its own development. 'Psychology' would then be a term whose early social significance had lapsed and been forgotten just as has been the case with many other names. Future students would then be mildly interested in the fact that their forbears thought that a psyche could actually be observed and described, but their own scientific labors would be uninfluenced by the term. We are not permitted, however, to take so optimistic a view of the future

satisfactoriness of the term. In the first place, at least so long as the term psychological science exists, giving an apparently solid foundation of fact to the assumed existence of a psyche, philosophers will speculate upon the nature of consciousness, and by so doing they will give moral courage through the mechanism of group suggestion to those who seek to found and perpetuate such a study. In the second place, popular charlatans will always gain ready access to the popular imagination by appealing to the psychic, with the result that scientific work will continue to be embarrassed by the general misconception of the field. The situation here is of much the same order as that which confronted the early chemists and late alchemists. As the chemists could work in an atmosphere freer from scepticism and distrust when they had freed themselves from the incubus of the term alchemy, so it should be expected, even in this enlightened age, that the investigation of human nature should be subject to fewer misunderstandings when the term psychology has been dispensed with. In the third place, the non-psychic studies have become so numerous at the present time, and the label 'psychology' is contemporaneously so misrepresentative, that new names for the science are coming into use. Of these Objective Psychology and Behaviorism are the most prominent. The former, however, is defective in that it suggests a sub-division of psychology and in that it contains the obvious contradiction of a non-psychic psychology. Furthermore, it fails to obviate the popular misunderstanding of the field created through quackish and literary usages of long standing and of great vitality. Among Americans, Behaviorism has proved a wonderfully apt term. It contains no suggestion of the psychic, and therefore possesses the many consequent advantages above suggested. It does, however, refer to an "ism" and is not, therefore, well suited to designate an entire science. The term Science of Behavior is too cumbersome, and the Greek equivalent Tropology is too reminiscent of a special problem in the behavior of lower animals, to make these acceptable names for the science. Again, the term Behavior is too general for the field of human behavior. This term includes many biological processes in which the scientific students of human nature are not vitally interested. The term Anthropology, science of man, escapes these difficulties, but it is preempted by a closely related discipline. Anthroponomy, the science of the laws which govern human action, is therefore chosen to designate the subject of the science of human nature whose chief followers have been listed as psychologists. Such a change in scientific nomenclature as this is, to be sure, established neither in a day nor by arbitrary decree. It is the outcome of common usage, and only the future can tell what the final

designation of the science will be. At present only one thing is certain, and that is the inappropriateness of the term 'psychology'.

We shall be greatly aided in our understanding of the nature and scope of anthroponomy if we compare it with related disciplines and then examine briefly its subdivisions. We shall thus be led to see that anthroponomy is set off from other disciplines not by a principle (such as the limits of consciousness) but by a content historically and practically determined. First let us consider sociology and education, which are related to anthroponomy in much the same way that this science is related to biology. We must first insist that the chief differences between the fields are to be arrived at through an examination of the activities of the investigators belonging to each, rather than through an analysis of the terms employed. From the standpoint of philology, *e. g.*, biology may be made to include all of the studies of living organisms. But from the standpoint of what the biologists are actually doing, the picture is quite different and will be sketched below. Sociologists, to return to our immediate problem, are concerned with field-observations of human social behavior. The procedures employed are as scientific and as thorough as are those found in economics and history, but there is the barest minimum of experimental analysis. Rather, the empirical data are gathered by observational methods. So far as an explanation of social behavior is sought in terms of the fundamental characteristics of human nature, sociology depends upon anthroponomy. Sociologists, therefore, make much use of such factors as instinct, imitation, the self, and thought in attempting to explain social phenomena; but few if any sociologists are directly concerned with increasing the precision of these fundamental categories.

The discipline of education is a broad field of theory, history, art, science, and administration. It overlaps anthroponomy in the scientific study of human nature under school-room conditions and in the study of the rating problems which arise in training in the school-subjects. This latter field is well developed under the title of educational tests and measurements, and it is historically and actually closely related in method to the fields of individual and industrial anthroponomy. The former field is as yet less clearly defined. Having been dominated by normal adult anthroponomy, it has hardly begun to attack its problems upon an independent basis. It still depends upon borrowing data gathered under laboratory conditions in anthroponomy, and it is insufficiently concerned with the impossibility of generalizing such data beyond the experimental conditions under which they were obtained. The treatment of the learning process is the most glaring example. Practically none of the

data gathered by psychologists and anthroponomists is applicable to the school system. Methods of learning which are most efficient under controlled laboratory conditions may very well fail to show this high value under the hurly-burly of schoolroom teaching and administration. *Most of these shortcomings would be overcome by treating the science of education as a branch of personnel management.* In this case the educator could feel free to attempt the solution of his own special problems unencumbered by efforts to *apply* some other science. Finally educational theory, so far as it deals with the scientific and not with the philosophic, turns to anthroponomy and biology for its fundamental conceptions.

We are particularly interested in the relationship existing between anthroponomy and biology because many critics have said that to eliminate consciousness from psychology is to turn the study of human nature over to the biologist. Let us analyze the problem and see what conclusion is to be drawn. Biologists turn to physics and chemistry for certain of their more fundamental explanatory formulations, although most biological phenomena cannot as yet be advantageously handled in this fashion and must be referred to proximate sources. Anthroponomy draws many of its explanatory rubrics from biology and may, therefore, ultimately secure them from physics and chemistry. As yet, however, not only does it not go so far afield, but the great majority of its explanations are evolved from its own domain rather than from the processes studied by the biologist. In addition to this contact with biology, there is a great similarity in the general character of the phenomena studied in the two sciences. This similarity we shall proceed to develop.

Biology may be subdivided into a plant and an animal science. Animal biology concerns man and the animals below him. Human biology is most specifically organized in the medical sciences with their underlying pure sciences of human anatomy, embryology, histology and physiology. Outside of the medical field and the above-noted closely related human sciences, animal biology is primarily working with the animals below man where the human entanglements are genuine but secondary. The field of animal biology is concerned with structure and behavior. These two problems shade one into the other and still represent a difference in emphasis which is justified by practical interest and by experimental returns. Structure and behavior may be approached from the standpoint of the adult organism or from that of ontogenesis and phylogenesis. In the latter case (phylogenesis) the science of genetics appears, dealing unfortunately at present with the genetics of structures to the practical neglect of the genetics of behavior. Human biology takes little account of the problems of the ontogenesis of behavior except in so far as

this behavior concerns disease-possibilities at various ages. Where the human biologist gathers data upon behavior as it occurs in infants, children and adults, he does this definitely under the guidance of medical problems and possibilities, and accordingly he neglects (1) the behavior which is directly involved in the adjustment of the individual to his environment, and (2) the coordination of the data gathered from the standpoint of development. He thus leaves his material in a form suited to support and further the diagnosis and treatment of disorders rather than in a form suited to the illumination of the problems of human nature. The studies of the human biologist take account (1) of the structure and structural relationships of the receptor and effector organs involved in behavior, (2) of the nature of the stimuli concerned, and (3) of the detailed quantitative and qualitative aspects of the response. Not all stimulus-response situations, however, are studied. Those selected concern either the stimulus-response possibilities of a single specific apparatus such as the eye, the ear, the tongue, the biceps muscles, etc., or the stimulus-response conditions within the organism involved in such activities as circulation, digestion, endocrine secretion, etc. The field of human biology has not interested itself to any appreciable extent in the adjustment of the human organism to its external social and non-social environment and, with the exception of psychiatry and some minor aspects of physiology, it has remained aloof from the so-called conscious processes. Even the animal biologists have quite generally avoided the problems of learning.

As human biology is set apart from the sciences of physics, chemistry, and bio-chemistry not by a difference in principles and points of view but by historically determined differences in subject matter, so human biology is set apart from anthroponomy. Inasmuch as none of the other sciences is sharply separated one from the other, it is to be expected that anthroponomy will be intimately related to its fellows. There is no general principle which sets off anthroponomical data from physiological data. Natural phenomena form a continuous series which is subdivided among the sciences by the processes of historical development working upon practical differences in subject-matter. We thus find anthroponomy dealing with those phases of human behavior which appeal to investigators as significant and which are not monopolized by other sciences. However, the field is not therefore one of odds and ends any more than are the fields of zoology, physiology, and anthropology which develop upon an equally practical and non-philological basis. For example, anthroponomy and physiology are both deeply interested in sensory processes. As a result, that problem is under constant investigation in one or the other field; but the relative proportion

of anthroponomists and physiologists engaged thereon is constantly shifting. At the present time industrial organizations, whose point of view is essentially that of physics and engineering, are much interested in vision and are perhaps doing the most work upon that process. To the extent that this is true, to that extent is the topic losing connection with anthroponomy and physiology. If the problems of learning, thinking, intelligence, custom, multiple personality, and others seem to be the inviolable property of the anthroponomist, it is only because historical conditions have not first led the human biologist seriously to extend his field to include these topics. Historically considered, subdivisions of labor have promised better results, although it should be added that the general scientific acceptance of a human psyche as involved in the above problems has tended to make many investigators avoid the field. In contrast to the above problems, those of reflex action, instinct, emotion, sensory processes, reaction-time, neural processes, crowd-behavior and others have seriously intrigued sciences other than anthroponomy. The problems have become, in the main, however, primarily anthroponomical in kind because of their more persistent attack from within this field. The broad aspects of natural phenomena are of such self-evident importance for scientific study that to the extent that one group of investigators deserts a given field, to that extent may another group be expected to take over the problem. And so one finds in an historical survey of any particular science contributions made by groups who are no longer represented in the field. This is significantly the case in anthroponomy, where the development of specialists to attack the problems of human nature and thus to free other men from the inner necessity of attempting it is still an urgent necessity.

If, then, there is no principle, such as the presence or absence of consciousness suggested by the psychologists, which divides anthroponomy from the other sciences, what are the fundamental problems which make up the science of human nature and which are not studied, or not intensively studied, in the other fields? The problems which are most characteristically anthroponomical are as follows. (1) *The ontogenesis and use of forms of behavior.* This includes such great topics as learning, thinking, memory, and work. (2) *Those stimulus-response situations which condition language-responses.* It is here that the psychologists have busied themselves with psychic processes and introspection. (3) *The analysis of behavior-samples with a view to the prediction of general and special performance.* This is the problem developed in the field of tests. And (4) *problems of inter-stimulation and response.* In contrast to these groups of problems, those of unlearned forms of response (reflex-action, instinct, and emotion), of sensory processes, of neural mechanism,

and of physical measurement of the individual, while of immense importance in anthroponomy, are nevertheless problems to whose solution significant contributions have been made and will continue to be made from outside the field of that particular science.

If we were to seek a definition of human nature which would satisfy the needs of scientific study, we should derive it from the data of human behavior. Human nature is most characteristically shown in three classes of adjustments: those which have a well marked developmental history in the individual's life-time, those which involve or otherwise come under the control of symbolic stimulus-response relationships, and those which involve the inter-stimulation and response of human individuals. Anthroponomy, as the science of the laws of human behavior, is therefore the science specializing in such human behavior as we have listed above. And as a science of human nature it specializes in those aspects of environmental adjustments which significantly define human nature.

Certain fundamental systematic problems of anthroponomy will be outlined later in the present paper. It is necessary at this point to indicate the subdivisions of the science into its various fields, and thereby to gain an understanding of the various points of view from which human nature is envisaged.

General anthroponomy may be subdivided as follows.

(1) *Physical* anthroponomy is concerned with the measurement of various organic characters such as the cephalic index, body length, weight, etc., and with the relationship of such measures to the constitution of races on the one hand and to behavior-possibilities on the other. The established science of physical anthropology specializes in the above problem of racial differences, while the work on the behavior-significance of physical traits has been developed by psychologists. It is upon the possibility of growth in the latter problem that the chief expectation of value in the field of physical anthroponomy is based. Unless the various physical measurements are related to individual or racial differences in behavior, they have no value for anthroponomy. This relationship may be remote and may involve no direct influence upon behavior. It may serve merely as a means of classification of individuals who in their turn show on the average certain significant behavior-differences; and yet unless the differences are related to behavior in the end, they possess no appreciable value for the science.

(2) *Cultural* anthroponomy is concerned with the nature and development of cultural products in the various groups and races of men. It is interested in the origin of various types of tools, houses, arts, religious and economic customs, etc., and in the dissemination of such phenomena over the face of the earth. As a field of endeavor, cultural anthroponomy deals chiefly with inferior races and with the cultural remains of vanished races, although there is no logical reason why its activities should not be extended to the present-day social activities of world-important groups. Cultural anthroponomy not only throws a significant light upon the genetic history of present-day customs, but it enables one to construct a fairly accurate account of the modes of behavior of early man. It is, however, largely a descriptive field of endeavor, inasmuch as its genetic explanations fail to fur-

nish a satisfactory account of the factors bringing about custom and change in custom. For this explanation recourse must be had to some of the following fields.

(3) *Social* anthroponomy deals with the inter-stimulation and response of human individuals. These relationships may be of varying degrees of complexity, organization, temporal and spatial extent. They may occur between two or more individuals of the same or different generations, and they may be relatively isolated instances of social behavior or cases of customary or institutional behavior. Inasmuch as it emphasizes contemporary human groups, social anthroponomy is not characteristically phylogenetic in its method. It deals in a descriptive and explanatory way with such well recognized phenomena as custom, tradition, mobs, crowds, audiences, leadership, the family, religion, fashion, and fads. It is concerned with language as a form of inter-stimulation and response and with the various uniformities in human behavior. It is closely related to the preceding field of cultural anthroponomy to the extent that materials from the lower and the vanished social groups are utilized. Social anthroponomy is as yet largely theoretical and descriptive. There is, however, an increasing tendency for it to become experimental in its method and for it to use the most recent explanatory conceptions developed in the study of human behavior considered irrespective of social significance.

(4) *Individual* anthroponomy is primarily concerned with individual differences in behavior. These problems involve differences in receptor capacities, in unlearned and learned equipment, and in general and specific capacities. The method of tests at present dominates the field, although training methods are equally applicable to its general problem. In general interest in this field is confined to differences which may be quantitatively evaluated.

(5) *Abnormal* anthroponomy is devoted to a study of all behavior which departs markedly from the norm. It thus deals with the very superior as well as the very inferior response. Its chief field lies at present in the abnormal forms of behavior which are not quantitatively measurable, *viz.*, the abnormal traits of personality and temperament, insanities, phobias, multiple personalities, dreams, etc.

(6) *Normal human adult* anthroponomy is the fundamental field of the science largely because of the fact that the normal human adult is the most accessible subject for experimental study. This field is concerned with an analysis of the normal human adult's behavior into its fundamental aspects.

The six fields of general anthroponomy which we have enumerated are among the most important of the organized subdivisions of the science at the present time. Any grouping of problems within the general field is arbitrary, however, and an even better insight into the nature and scope of the science of human behavior can be secured by considering the various aspects of the human individual which have been emphasized as points of organization for the subdivision of labor within the science. Individuals share alike an animal ancestry. They differ in age, sex, race, culture, nationality, in their physical and character traits. They may be studied with reference to a single trait analytically, genetically, or comparatively from trait to trait. Thus there may be, and is, a genetic anthroponomy, a comparative anthroponomy, as well as a racial, animal, social, and national anthroponomy, and an anthroponomy of personality, sex, infancy and childhood. Indeed circumstances may

even justify the terms laboratory, clinical, and field anthroponomy. Human nature, as we have defined it, can be adequately understood only when studied in these various significant aspects,—any one of which may be made a legitimate subdivision of the general science.

Let us now turn from a statement of the character and scope of general anthroponomy to a brief indication of its most important systematic problems. There are certain of these problems which at the present time command an attention only less absorbing than the problems of experiment and observation. A large portion of this interest is of a temporary character and grows out of the transition which is taking place from the psychological to the anthroponomical point of view. A certain portion of the interest, however, is permanent and concerns the theoretical problems which are inevitable in the systematization of any science. These permanent theoretical problems are concerned with the fundamental conceptions which constitute the skeletal frame-work upon which empirical material is placed. Conceptions of this character are abstractions made from the continuum of empirical data. They are thus artifacts, and their value depends upon their scientific usefulness. Atoms, electrons, light, heat, sound, and magnetism are illustrations of such abstractions in physics. The introspective psychologist utilizes such arbitrary divisions of the observable continuum as sensation, perception, memory, conception, volition, and thought. General anthroponomy likewise regards certain rubrics as particularly important and groups its materials under their names. We shall see later in detail what these are, but at present we must turn back to comment briefly upon that large interest in systematic anthroponomy which has been called temporary in character.

The systematic problem which is most prominent in contemporary discussion is that of the subject-matter of the science. This problem arises in psychology rather than in anthroponomy and concerns the question of consciousness. In preceding papers[1] we have discussed this topic in detail, and at that time attention was called to the three chief psychological points of view of structuralism, functionalism, and behaviorism. No psychologist has ever denied the existence of consciousness conceived as a different aspect of the universe from that constituted by matter, unless the behaviorists should be called psychologists. Philosophers and devotees of other scientific fields have done so at times since the days of the ancient Greeks; but the psychologist has clung to the position that the psyche constitutes the essential

[1]W. S. Hunter, The Problem of Consciousness, *Psychol. Rev.*, 31, 1924, 1-31; The Symbolic Process, *ibid.*, 478-497.

aspect of human nature. The behavioristic psychologists have repudiated this position and thereby in fact, but not in name, have constituted themselves anthroponomists.

If consciousness and the bodily processes closely related to it constituted the subject-matter for study, various additional systematic questions would arise at once. (1) What is the relation of mind and body? (2) What methods are available for the study of consciousness? (3) What are the essential attributes of consciousness? (4) What are the constituent elements of consciousness? (5) How may the presence of consciousness be objectively determined? (6) What is the function of consciousness in the biological process? (7) How are the elements combined to form the complex conscious processes? (8) Into what combinations do the complex conscious processes enter? (9) Is the subconscious a verifiable datum? These fundamental systematic problems of the psychologist need not all be solved by the anthroponomist; but they must be absorbed and not ignored in his science, just as room has been made in chemistry for the observational data out of which the problems of the alchemist grew.

The concern over the subject-matter of the science of human nature can be permanently allayed only as a result (1) of an accumulation of experimental data which will give unambiguous evidence of what is being studied; and (2) of an understanding of the nature of consciousness derived not from philosophic or religious prejudice but from its actual empirical value in the scientific study of man. The problem of consciousness and the systematic questions growing therefrom cannot be successfully ignored. As we have just said, they must be absorbed into the science. It has been our purpose elsewhere to attempt such an absorption of the psychic problem into the field of anthroponomy. In the present paper we may therefore pass over these questions and attempt an enumeration of the systematic problems of anthroponomy. These may be listed as follows.

1. The nature of the subject matter. Three particular problems, each of which has numerous sub-heads, are to be noted here:
   (*a*) Is consciousness a separate aspect of human nature?
   (*b*) What are the essential characteristics of human nature?
   (*c*) What is the pragmatic relationship of anthroponomy to the other sciences?

II. The general methods of gathering data in anthroponomy.
   (*a*) Field observation, clinical observation, experiment.
   (*b*) Introspection as a possible method.

III. Explanatory technique.

IV. The fundamental classificatory conceptions in anthroponomy.

The following list is suggestive, but it is in no sense exhaustive. Since the items are not logically determined, such a list is in constant revision.

1. Unlearned stimulus-response connections.
2. Learned S-R[2] connections.
3. S-R relationships in which two or more individuals are vital elements: custom, fashion, mob, tradition, crowd, audience.
4. Symbolic and non-symbolic S-R relationships.

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[2]S-R = stimulus-response.

5. S-R relationships under (and not under) the organism's control through the self-excitation of receptors and possibly through central arousal.
6. Integration and disintegration of S-R connections.
7. Retention, re-instatement, recognition.
8. Controlled and uncontrolled, purposeful and non-purposeful, sequences of S-R relationships (free and controlled association).
9. Stimulus and response.
10. Interference and transfer.
11. Innate capacities, special abilities.
12. Irreversible sensory process-language response (SP-LR) relationships[2] (sensation, attention, images, emotions, etc.).

In connection with the fundamental classificatory categories, it is worth noting that in the transition from psychology to anthroponomy the fields of animal and individual psychology have had a leading rôle. This is chiefly because these fields have specialized in the objective (non-psychic) study of behavior. And yet, although the field of normal human adult psychology has been dominated by the shadow of the psychic, one should not ignore the objective character of much of the experimental work that has been accomplished in this field, the topics of sensory processes and learning being particularly important. It is probably because of these facts, *plus* the importance which attaches to the method of experiment, that the classificatory rubrics of general anthroponomy are chiefly drawn from these three fields of study.

Since we have already discussed the problem of consciousness in the articles already referred to, we shall confine ourselves in the remaininder of the present paper to a brief examination of the second and third systematic problems above listed.

Field observation and clinical observation were given with experiment as two of the three methods of gathering data in the science of anthroponomy. Concerning experiment, nothing further need be added. The principle of experiment is the same in all sciences, and involves the artificial control and repetition of conditions for the accurate analysis and description of phenomena. Such experimental methods are employed most widely in normal human adult, individual, and comparative anthroponomy. They are being introduced on a small scale in the fields of abnormal and social anthroponomy. In contrast to the controlled and quantitative character of these methods stand the uncontrolled and qualitative procedures of field and clinical observation. Field observation is a term borrowed from biology to designate the method of observation of subjects free in their natural habitat and undisturbed by experimental interference. In biology the method is best exemplified in studies of homing,

[2]For the nature of these relationships see "The Problem of Consciousness" and "The Symbolic Process."

migration, reproduction, feeding, and other similar activities; in other words, in the great field of habits and instincts as studied by the naturalist. These studies have great value partly in their suggestiveness of specific problems and controls for experimental analysis and partly in the historical, sympathetic, and cultural view which they afford of the animals concerned. In anthroponomy this method of field observation, with the same values and defects, dominates the subdivisions of cultural and social anthroponomy and is essentially the method employed in physical anthroponomy. It is also employed in the compilation of case-histories in individual and abnormal anthroponomy. Clinical observation, although non-experimental and qualitative, differs from field observation in the following fundamental ways. (1) The subject is removed during observation from his natural habitat to special surroundings in which the observer is a dominant factor. (2) Through direct observation of the subject's behavior and through observation aided by measurement, the observer seeks a diagnosis of the subject's relative status of personality. (3) Because of the comparative point of view and the insistent need to evaluate the subject's behavior, clinical observation is less objective and is more open to the influence of prepossession than is field observation. This defect, it should be noted, extends beyond the fields of individual and abnormal anthroponomy wherever the method of personal interview, unaided by objective standards, is found. And (4) the method of clinical observation is typically an individual method of observation. In conclusion it should be pointed out that clinical observation accompanies much experimental work and furnishes a qualitative record of the subject's attitude and peculiarities of response. The typically qualitative character of the data gathered by field and clinical methods is undoubtedly largely responsible for the shortcomings of the fields concerned in the development of fundamental contributions to the classificatory concepts of anthroponomy.

In concluding this brief account of the character of general anthroponomy and its systematic problems, a few words must be added concerning the explanatory methods of the science. Explanation seeks to give the determining conditions for whatever phenomenon is under consideration. It always falls short of completeness, and it never pretends to be ultimate. The choice between the more and the less nearly ultimate is one to be made upon the basis of practicality conceived in terms of control and verification by the experimenter. There is, therefore, no one kind of explanation which can be called *the* method of explanation in anthroponomy. In general three types of explanation are available: explanation by analysis into component parts, by formulation of the genetic conditions, and by reference to receptor-conductor-effector mechanisms.

These three types are not always equally practicable. Where they are, the third method of explanation in terms of the individual's action-system is the most nearly ultimate. As the progress of science makes possible a physico-chemical explanation of receptor-conductor-effector activities, it is to be expected that this additional type of explanation will be employed, as is indeed even now the case in many physiological studies. We may illustrate the problem of explanation as it arises in anthroponomy as follows. The verbal response "box" following upon visual stimulation may be explained: (1) by an analysis of the response into its component elements through the analysis of the sound waves produced or by the registration of tongue and laryngeal movements; (2) by a statement of the number of trials, the total time, etc., of the learning process which was necessary in order that the behavior might occur; and (3) by a statement of the receptor activities which initiated the neural impulse, of the synaptic conditions present, and of the resulting effector activities. It is rarely possible at the present stage of neurological knowledge to make the third type of explanation anything other than suggestive when it is applied even to moderately complex behavior. This form of explanation, however, is approximated by stating the natures of the stimulus and the response and by filling in the neurological data largely by arm-chair methods. If we change our illustration from that of a verbal response to that of a custom, the third type of explanation may at once be ruled out as an actual method now employed. The explanations attempted of custom are in terms of analysis into component responses and in terms of genesis. This is also true of the explanations offered for many other phenomena, *e. g.*, pathological personalities. Perhaps the most prevalent form of the explanatory question in the science of anthroponomy is this: what are the historical conditions of the behavior in question? When we said earlier in the paper that anthroponomy is essentially genetic in its point of view, we referred primarily to the systematic attitude adopted toward its subject-matter. Here we wish to stress the point that anthroponomical explanations are also largely genetic, involving to a very large extent—as they do—accounts of the previous training of the subject as well as attempted descriptions of inherited probabilities of response. To this genetic explanation of a given phenomenon, neurological data are added where they are available.

It is worth noting that the psychologist has inevitably been compelled to resort to the scientific methods of explanation outlined above in order to escape from theories of the psychic causality and control of conduct. In like manner the alchemists were led to chemical explanations as they divorced themselves from the control of their magical prepossessions. When the psyche alone is considered, the psychologists have sought to explain consciousness either by a structural analysis or by a psychic causality, although some of them have taken the theoretically invalid position that the psyche can be explained in terms of neural process (see above, p. 287). The structural psychologists have thought that this latter method could be followed without abandoning a psychophysical independence; but the functionalists in their development of the field, although paying lip-worship to the doctrine of psychophysical parallelism, have made extensive practical use of a causal interaction of the psyche and the biological process. No adequate neural or genetic explanation can be given for the problems which are conceived to arise in the realm of the psychic.

## Retrospect

How shall new wine be kept if poured into old bottles? Old containers must be replaced by new as fundamental novelties in prospective content appear. He would be a foolhardy merchant indeed who allowed his business to suffer because of a sentimental fondness for the boxes and casks of an old trade!

Words are tools indispensable to a scientist's trade, but if the tools be broken or dulled, wherewith shall the trade be nicely prosecuted? That which concerns the worthy craftsman is the fashioning of his beautiful object rather than the conservation of his broken tools and loyalty to his historic guild.

Scientific students of human nature grow restless under the confining implications of the term 'psychology' and under the implications of a magical insight attributed to them by an eternally credulous public. Anthroponomists do not fear a loss of corporate individuality through absorption into biology because an historically determined content in a great field guarantees the future.

# BOOK REVIEWS

*Psychologie vom empirischen Standpunkt.* By FRANZ BRENTANO. Mit ausführlicher Einleitung, Anmerkungen und Register herausgegeben von OSKAR KRAUS. Erster Band. Leipzig, 1924, F. Meiner. pp. xcvii., 279

In the reissue of Brentano's *Psychologie* Dr. Kraus, who is professor of philosophy in the University of Prag, has rendered a notable service to all students of psychology. For many years past Brentano's book has been—not difficult to procure, not procurable only at an exaggeratedly high price, but simply and flatly unprocurable. This fact alone might have prompted its editor to think twice before penning the second sentence of his preface: "the scientific world is only gradually becoming aware that the work was epoch-making." A whole edition does not disappear out of sight, and copies in private libraries do not resist the temptation of American gold, unless the book in question is quite especially valued. Nor would it be difficult to show that the psychologists of thirty years ago—Kuelpe in his *Grundriss*, Stout in his *Analytic Psychology*—were fully alive to the importance of Brentano's doctrine.

The new issue is to form two volumes of the *Philosophische Bibliothek*; only the first volume (192) is now before us. There is good reason for the division, since Brentano himself published in 1911 a corrected reprint (with additions) of the last five chapters of his book (*Von der Klassifikation der psychischen Phänomene*, Leipzig, Duncker & Humblot. pp. viii., 167). The volume which I am here noticing gives us, then, the *Psychologie* down to the end of Bk. ii., Ch. 4. The second and as yet unpublished volume will give us the contents of the *Klassifikation*, the essay *Miklosich über subjektlose Sätze* (1883), five posthumous memoirs from Brentano's papers, and a much-needed index. So, for about five dollars, Brentano's work may henceforth be obtained by our graduate students. It is perhaps worth remarking, in the light of what I said above, that the new issue has not brought on the market—so far as I have been able to learn—a single copy of the original edition.

Dr. Kraus has written for the present volume, besides a general preface and a bibliographical list of Brentano's publications, an elaborate 75-page Introduction to his master's teaching. The chapters are entitled: (1) Brentano's distinction of 'descriptive' and 'genetic' psychology and his relation to phenomenology and theory of object; (2) Brentano's doctrine of the 'psychical relation' in its historical development; (3) Brentano's doctrine of the real as the exclusive object of our consciousness; (4) Brentano's attitude to psychologism; (5) the doctrine of outer perception; (6) Brentano's use of the expressions 'psychical' and 'physical' phenomenon; and (7) the method of descriptive or phenomenological psychology; inner perception and inner observation. All these discussions are of value; the second, third and fourth refer us directly to the appendix of the *Klassifikation*; but the mingling of exposition and controversy makes them difficult reading for a beginning student of Brentano. Still, nobody who is afraid of difficult reading will attempt Brentano himself.

I shall not try in this place to give any further account of the Introduction; its various topics need far more space than is proper for a review. There is, however, one point—a point made by Dr. Kraus at the very beginning—which seems to demand comment. The editor states positively that the *Psychologie* of 1874 contains aprioristic elements. After showing that descriptive psychology (the *Psychognosie* of Brentano's later classification) advances at a bound and without any sort of induction from general

concepts to general cognitions, Dr. Kraus writes: "These apodictic (always and wholly negative) cognitions, that are directly evident from the concepts, are however already known to the *Psychology* of 1874. 'Nothing is judged that is not ideated,' 'No loving or hating without ideation of that with which we are occupied emotionally,'—these, and many other psychological cognitions of which the *Psychologie vom empirischen Standpunkte* . . . treats, bear an aprioristic character" (xvii.f.; cf. lx., 268, 276). I do not, of course, deny Brentano's apriorism in the late eighties. I am strongly of the opinion, however, that there is no conscious trace of it in the *Psychologie*; if there were, Brentano must have been either deceitful or stupid,—and one hardly knows which adjective suits him the worse. For he says in his Preface: "My standpoint in psychology is the empirical; I recognize experience alone as my teacher"; and when he adds that as philosopher he entertains *eine gewisse ideale Anschauung*, he is at pains to declare that he finds it compatible with 'this standpoint' (this edition, 1). Moreover, the three chapters on psychological method are concerned with its experiential basis, with induction, and with deduction and verification; there is no word of the 'jump' made by descriptive psychology. Thirdly, the passages to which Dr. Kraus refers (112, 225, 227) appeal obviously either to experience or to argument; and the negative formulas are obviously rhetorical and not essential (cf. the positive formulas, 112, 120, etc.; and for argument cf. the 'demnach' of 120 and the 'dargetan' of 225). I cannot find, then, that there are aprioristic elements in the *Psychologie*. Indeed, on the basis of my own experience, I must go further; I doubt if a student of the *Psychologie* is prepared by that study for the part played by the *a priori* in Brentano's later thinking. I did not happen to read the tract *Vom Ursprung sittlicher Erkenntnis* (1889) until some time after I had become familiar with the *Psychologie*, and its *Wertaxiomatik* came to me as something altogether new.

So much, at present, for the Introduction. The text is, of course, a reprint of that of the original edition. Not much has been done in the way of 'textual' revision; *Tätigkeit* has been replaced by the correct *Fähigkeit* on p. 181; but the offending *als* has not been removed on p. 14, l. 9; on p. 65 the two sentences "Doch sind . . zuzuweisen" should have been enclosed in a parenthesis—else they are hardly intelligible; and so on. Dr. Kraus has, however, put in squared brackets those passages (cf. esp. 117, 202, 204, 219, 222, 230) which contain doctrine that Brentano himself has later rejected or corrected. The indications are useful; one wonders only why the "worunter hier nicht eine Realität zu verstehen ist" of pp. 124 f. has been allowed to stand unmarked (cf. 269 and *Klassifikation*, iv.). The running heads do not give the corresponding page-numbers of the first edition, and the running head of the recto does not give the section-number of the chapter. The footnote on p. 190 should be deleted.

The volume ends with some 25 pp. of Notes by the editor. The student will find these Notes exceedingly valuable. They clear up the meaning of doubtful passages, indicate shifts of terminology, furnish cross-references and outside references to related work, and provide in general just the sort of commentary that the beginner needs. Unfortunately, their indexing is of the one-way sort; the pages of the text refer to them, but they do not refer back to the pages,—though one may very well wish to turn from a given Note to the passage of the text which the Note illustrates.

Mechanically considered, the volume leaves a good deal to be desired, and the proof-reading of the Notes has been hasty; I have, however, not found any error that cannot at once be corrected by the reader. The book is light, and of the right size for a pocket.

E. B. T.

*Helmholtz's Treatise on Physiological Optics, translated from the Third German Edition.* Edited by JAMES P. C. SOUTHALL, Professor of Physics in Columbia University. Vol. I. The Optical Society of America, 1924. pp. xxi., 482.

Helmholtz was born in 1821. The Optical Society of America decided to celebrate the hundredth anniversary of his birth by publishing an English translation of the *Handbuch der physiologischen Optik*. It is curious that the work has never before been translated: possibly its length was a deterrent, perhaps there was no A. J. Ellis to undertake it, perhaps there was no guarantee of academic sales such as made the translation of the *Tonempfindungen* a promising commercial venture. We are now, at all events, to have the book in English; the first volume, Anatomical Description and Dioptrics of the Eye, is before us.

The editor has decided, and I think rightly decided, to make the English version essentially a reproduction of the third German edition. The German editors faced a very difficult task. They chose, as we all know, to ignore the second edition, and to reprint (with supplements of their own) the practically unchanged text of the original work of 1867. This was to cut the knot, rather than to disentangle it; but they adopted the more heroic measure with their eyes open, and their course has been generally approved. Psychologists, of course, must still make use of the second edition as well as of the first; and that is something of an inconvenience. It is, nevertheless, difficult to see how the second edition could have been converted, in reasonable space, into a Helmholtz memorial of the sort intended by the sponsors of the third.

The first volume of the English translation has as frontispiece the portrait of Helmholtz which appeared in the third volume of the German original. It contains also (443-482) a new supplement on ophthalmoscopy, reprinted from A. Gullstrand's *Einführung in die Methoden der Dioptrik des Auges des Menschen*, 1911. It has, further, a number of explanatory and corrective footnotes, initialled by the editor and his associates; these notes are crisp and authoritative—let us hope that the corresponding notes in the other two volumes will be of like nature! Aside from these additions, the book is a reproduction of the first volume of the German third edition.

The translation is loose, looser perhaps than it need have been, though in the dozen passages that I have compared with the German text there is rarely any serious distortion of meaning; every now and then, however, the translator slips, as in the sentence about Carion on p. 257. Technical terms —the technical terms of the psychophysicist—come off at second best. Our familiar dispersion circles appear as 'blur circles' or 'circles of diffusion' (121), although the word *Dispersion* occurs in Helmholtz' own index, and although both Baldwin's Dictionary and the new Century Dictionary give the accepted usage. The *Visirlinie*, which is the sighting line, is rendered 'line of sight' (124), and *Gesichtslinie* is translated 'visual axis' (97). *Gesichtsfeld* becomes 'field of view' (93). Fortunately, all these terms are rightly translated in Baldwin's Dictionary, and the Dictionary has had time to make its influence felt.

The *Optik* is a serious book, and the translators have taken it seriously. I do not think that the phrase "this youthful and at the same time effervescing field of knowledge" (viii.) could have been penned by anyone who was not in such deadly earnest that his sense of humor was in abeyance. "Concentric circles . . . very close together but at equal distances apart" (191) suggests the tall but respectably dressed stranger. The passage about the *Zolltel* (138) is left blind for the reader who does not know German. Helmholtz might have been allowed his little joke ('inchth' is, after all, not much less euphonious than 'twelfth')—especially since the joke did not go too far.

I have touched only on minor points; the volume as a whole does not fall within the competence of the psychologist. The remaining two volumes will call for fuller treatment. E. B. T.

*Intelligenz und Wille.* By E. MEUMANN. 4te verbesserte Auflage, herausgegeben von G. STOERRING. Leipzig, Quelle & Meyer, 1925. pp. xii., 360.

Meumann published the first edition of this book in 1908. In the following year, Wundt reviewed it in the *Psychologische Studien.* Wundt wrote more in sorrow than in anger, but his sorrow expressed itself in some fairly severe criticism. Meumann, apparently, was not impressed; in the preface to the second edition of 1913 he says that few of the many reviews that the book received have helped him, and he makes no mention of Wundt's. After Meumann's death in 1915 the care of the book fell to his friend Professor Störring, who brought out a third edition in 1920 and is responsible for the present, fourth, edition of 1925. Professor Störring has wisely confined himself to the writing of notes—defensive, critical, illustrative—appended to the various chapters. He takes his stand, in general, as defender of Meumann's views against the attacks or counter-opinions of contemporaries.

The *Intelligenz und Wille* is, of course, a contribution to the psychology of personality, differing from most essays of its kind in that it seeks to go back to 'psychical elements.' As these elements, however, are mainly 'types' and 'capacities,' the book has a distinctly popular flavor.

The editor might, perhaps, have removed one or other of the identical phrases on pp. 222, 230. The repetition is natural enough for Meumann, who wrote under stress of emotion, but there is no reason to retain it in cold blood. The subject-index seems to have been revised, though it has some mistakes. The index of names is quite untrustworthy, both by mistake and by omission. The references given to Wundt are 120, 138, 224, 344, 345; I make them (and there may be others) 115, 132, 206, 221, 222, 224, 225, 230, 330, 344, 345. Helmholtz and Herbart, *e. g.*, do not figure in the index; nor does Wentscher—curiously, since Meumann himself mentions her in his preface of 1913.

E. B. T.

*Dynamic Psychology.* By THOMAS VERNER MOORE. Philadelphia, London and Chicago, J. B. Lippincott Company, 1924. pp. viii., 444.

Two tasks are attempted in this text: (1) the venturing of a synthesis of behaviorism and introspectionism, and (2) the meeting of a growing demand for a systematic presentation of the dynamic point of view in psychology.

The first task is approached with a definition of psychology as the science of human personality. More or less adopting Kant's tripartite division of the mental life into knowing, feeling and doing, the author supplements introspection with behaviorism in this wise: he presents the cognitive aspect of mind introspectively, while the conative and affective aspects are considered from both the introspective and the behavioristic standpoints.

The second task is commenced with a positing of impulses as the driving forces of human nature. Here follows a subtle differentiation of impulses from the instincts with which they may be easily confused. Impulse is defined as a tendency to action of which we are focally or marginally aware prior to the action itself. In other words, it is a conscious or subconscious urge to action of a specific sort. At once impulse assumes the rôle of a conscious element destined to initiate action of some sort. An impulse precedes in consciousness every unit action. Consequently, the number of impulses is unlimited. On the other hand, the various so-called human instincts, like the various so-called human habits, are but general names for large groups of impulses subjectively considered.

Impulses are divided into motor, sensory, affective and intellectual, according to the part of the personality which stands in need of exercise. Here impulses appear familiar to the student of introspection as simple feelings which activate us. The inhibition of a lively impulse transforms impulse into desire or craving. Impulses often conflict, and the stronger prevails; for it is assumed that these elemental impulses vary in strength.

Having thus grounded dynamic psychology, Moore proceeds to recapitulate the psychoanalytic doctrine of wish. It must be added that to the discoveries of Freud, Adler and Jung the author contributes something of his own.

The closing chapters of the book are devoted to a critical analysis of volition, from which the reader emerges somewhat confused. The author, owing to his psychodynamic premises, is compelled to refute James' ideomotor theory of volition and also Woodworth's theory of conceptual control. A voluntary act, he admits, is preceded by an idea or thought of the action to be performed, but this idea or thought is not the cause of the act. Prior even to the idea and provocative of it is the impulse, the veritable creative fiat.

To the freedom of the will Moore subscribes with certain reservations. We are free in the sense of being at liberty to choose the means that make us happy; while, on the other hand, we are compelled by nature to seek to be happy. He would emancipate us from the tyranny of external circumstance, but not from that of our internal drives.

The book has appended to it a glossary of technical terms. Its style is simple and fluent; its logic is somewhat intricate.

West Virginia Collegiate Institute F. C. SUMNER

*Introductory Psychology for Teachers (Revised).* By E. K. STRONG, Jr. Baltimore, Warwick and York, Inc. 1922. pp. ix, 255.

In this text, Strong, the author of *The Psychology of Selling Life Insurance*, has brought forth such a mass of material illustrative of the many problems in education that it is rather difficult to get down to the fundamentals. Perhaps, however, one should not properly speak of illustrative material, for the writer is not merely using illustrations to clarify definitions and theories, but has so planned the work upon the inductive method that the student may work ahead and develop his own concepts. "The even numbered lessons present problems to be solved and the odd numbered lessons supply in a general way answers to the problems, together with a broader interpretation than the average student will discover for himself." As the book is designed for prospective teachers who have not studied psychology and who will not pursue the subject farther, the results of using such a method will probably be satisfactory enough. The reviewer finds it very difficult, however, to start out without a few guiding concepts.

The psychological interpretation is based entirely upon the situation-bond-response theory.

*Angewandte Psychologie: Methoden und Ergebnisse.* By ERICH STERN. Leipzig, B. G. Teubner. 1921. pp. 124.

This little manual is a convenient summary of the methods and results of Applied Psychology. It covers the usual subjects of intelligence tests, criminal psychology, work and fatigue, industrial and vocational psychology. For one who wishes a short account of the status of Applied Psychology in Germany it will prove fairly satisfactory.

*Methodensammlung zur Intelligenzprüfung von Kindern und Jugendlichen.* By WILLIAM STERN and OTTO WIEGMANN. Zweite stark vermehrte Auflage. Beiheft 20, Zeitschrift für angewandte Psychologie. Verlag von J. A. Barth, Leipzig, 1922. pp. iv, 316.

This monograph upon tests and test-methods should prove valuable to those interested in tests. In the past ten years, many new tests have been developed in Germany and few of them have become known in this country. The more recent tests in Germany have this point of difference from American tests: in Germany, more attention has evidently been given to the psychological background of tests, whereas we have been more concerned with the statistical treatment—our tests are more empirical.

The authors give a classification of both single and group tests with a complete bibliography. Although the attempt has been made to include all of the work upon this subject, many of the American tests have escaped attention.

*Schriften zur Psychologie der Berufseignung und des Wirtschaftslebens.* Edited by OTTO LIPMANN and WILLIAM STERN. Published by J. A. Barth, Leipzig.

Heft 11 **Methoden zur Auslese hochwertiger Facharbeiter der Metallindustrie.* By OTTO LIPMANN and OTTO STOLZENBERG. 1920. pp. 79.

" 12 **Kurzer Bericht über Arbeiten zu Eignungsprüfungen für Flieger-Beobachter.* By W. BENARY. 1920. pp. 61.

" 13 *Fragen zur Erforschung des Wirtschaftslebens der Naturvölker.* By RUDOLPH STEINMETZ.

**Die Wirtschaftspsychologie und Berufseignung in Überseeländern.* By HANS HENNING. 1920. pp. 52.

" 16 **Untersuchungen über die Fehlleistungen beim Maschinenschreiben.* By WILH. HEINITZ. 1921. pp. 17.

" 17 **Die Berufseignung des Damenfriseurs.* By R. W. SCHULTE. 1921. pp. 77.

" 18 *Die Berufseignung des mittleren kaufmännischen Bureaubeamten im Buchhandel.* By JUSTUS STRELLER. 1921. pp. 61.

" 19 **Die Monotonie der Arbeit.* By HERBERT WINKLER. 1922. pp. 45.

" 20 **Bibliographie zur psychologischen Berufsberatung, Berufseignungsforschung und Berufskunde.* By OTTO LIPMANN. 1922. pp. 60.

" 21 *Die Münchner Eignungsprüfung für Buchdrucker und Schriftsetzer.* By ALBERT HUTH. 1922. pp. 28.

" 23 *Taylorsystem unter schwerer Muskelarbeit.* By HEINRICH DOEVENSPECK. 1923. pp. 38.

These monographs (those marked by an asterisk are off-prints from the *Zeitschrift für angewandte Psychologie*) are from the series edited by Lipmann and Stern. The general purpose of the series is the scientific study of vocational and economic psychology and its applications to the fields of social, economic and pedagogical organization. In general, the investigations are well carried out and interestingly presented.

The investigations dealing with specific trades, such as the metal industry, aviation, printing, etc., are of a more definite value. A careful analysis—a job-analysis—has been made, and various experimental methods adapted to the measurement of the traits necessary in the particular trades have been ingeniously devised. Vocational psychologists and employment managers will find these monographs, and also Lipmann's Bibliography (Heft 20), both useful and suggestive.

It is interesting to note that the interest in vocational psychology and the use of diagnostic tests in employment work depends to a certain extent upon the condition of the labor market. Vocational and employment psychology received a good deal of attention in this country a few years ago, when there were a good many unemployed. More recently, less interest has been manifested in this field, and much of the work undertaken by corporations, industries, etc., has either been curtailed or abandoned. With the present unemployment in Germany, a similar demand for precise methods in the selection of employees and for trade studies is evidently making itself felt.

Cornell University SETH WAKEMAN

## NOTES

### EXPERIMENTS ON THE COMIC

In the time that has elapsed since my paper "Laughter, a Glory in Sanity" appeared in the JOURNAL for July 1922, I have seen no comment on my theory, and the subject is doubtless of slight current interest to psychologists, who are engrossed in more urgent problems. Presumably no one has gone so far as to attempt to devise tests for the theory. I have tried to think for myself how it might be tested, and I here set down my ideas.

The theory, if true, implies the following corollaries.

(1) Everything comic states or suggests something perceived as a falsehood.

(2) Nothing conveying a falsehood is thereby comic, so long as it deceives.

(3) Nothing conveying a falsehood is thereby comic, unless it tends to deceive, or, in other words, unless an effort of judgment is needed to perceive the falsity.

(4) Anything conveying a great enough falsehood that is suddenly perceived as such by an effort of judgment is comic (for a free and susceptible mind).

(5) Of two things conveying falsehoods perceived as such by equal efforts of judgment, the one conveying the greater falsehood is the more comic.

(6) Of two things conveying falsehoods of equal degree, the one requiring the greater effort of judgment to perceive the falsity is the more comic.

The first two corollaries contain, in different terms, the substance of Schopenhauer's analysis of the comic. He said that the source of the comic is the incongruity between what we think and what we perceive. M. Eastman, in "The Sense of Humor," remarks that this is merely a philosophic way of describing an error,—I should say rather, a detected falsehood. Most or all writers on laughter recognize incongruity as the base of the comic but I have found none who has pointed out that incongruity is simply the opposition of what we consider as true and what we perceive as false, except in so far as this is implicit in Schopenhauer's way of putting it. This seems to me to be the reason for the failure of all, including Schopenhauer, to explain why incongruity should excite a pleasant emotion.

The words "effort of judgment," in the propositions given above, contain the crux of whatever is new in my theory. If it could be proved that an effort of judgment always occurs when the sense of the comic is aroused, and that the sudden rejection of a sufficient falsehood by an effort of judgment

will always arouse a sense of the comic in an open mind, it would then be manifest, I take it, that the agreeable quality of the emotion is associated in some way with the power of judgment. If it could be proved further that the intensity of the emotion varies directly with the effort, and also with the degree of discrepancy overcome by the effort, the final inference that the emotion is pleasure in the power of judgment would seem to me unescapable.

Tests of the theory, therefore, might be directed toward determining whether an effort of judgment is thus related to the sense of the comic, in the rejection of falsehoods. The task may be formidable, because the "effort of judgment" thus involved in the theory is by no means always on the surface of consciousness. It does not mean obvious effort, necessarily, but a relatively strong exertion of the faculty that works abundantly and unceasingly in every conscious moment to interpret and correlate experiences. Obviously, such an exertion is not readily to be caught and measured in the act. But we can study at leisure the material on which it works, and we can select for this study matter that has been stripped of unessentials.

My idea of a working method would be to take a random series of bald statements contrary to familiar fact, to make actual tests of their relative comic value, and then to study them to see whether these relative values can be accounted for by corresponding degrees of falsity and deceptiveness. I would first ask a friend, uninformed of my purpose, to get me up a list of (say) 20 "brief, snappy lies," without other restriction. Or perhaps it would be better to get one lie each from a number of persons. I would write these false statements on numbered cards and submit the pack to different persons, preferably to bright children 12 to 15 years old, asking them to arrange the cards according to the degree in which the statements appealed to them as funny. After each such test, of course, due record would be made and the cards shuffled. If the result was a fair degree of uniformity, an attempt at analysis might prove profitable.

When a large number of falsehoods had thus been valued and analyzed, I should hope to have acquired a better understanding of the exact nature of the element of plausibility or deceptiveness, and also of just what I mean by "degree of falsity." I would then attempt a final test by trying to devise falsehoods that would conflict with the conditions posed in my six corollaries, especially the fourth.

I say I would do these things, but I do not propose to. My technique would be too crude. My hope is that some skilled experimenter may find meat enough in the suggestion to translate my rough ideas and lay language into proper psychological terms, and undertake to prove me right or wrong.

A single instance may help to show that there is profit for the student of the comic in the scrutiny of barefaced falsehoods. It will be recalled that among the 'many things' of which the Walrus proposed to talk with the Carpenter were

> "Why the sea is boiling hot
> "And whether pigs have wings."

These lines still hold the profoundly comic flavor that they had for me as a child. Yet it is not particularly funny to say "The sea is boiling hot," or "Pigs have wings." What is the difference? These bare extravagances have simply been wrapped in a kind of plausibility by suggesting that one is a familiar but unexplained fact and the other a doubtful subject for debate. The lines gain a heightened effect, of course, by immediately following so genuinely rich and stimulating a list of topics for conversation as the classic "shoes and ships and sealing-wax, and cabbages and kings." I do not know what primal longings may be satisfied by utterance of the lines that I have quoted, nor what elements of social correction or palliation of grief they may contain. But I am sure they give me pleasure, and I believe this pleasure, in its plain and palpable aspect at least, is nothing more than recurring delight at not being fooled by them.

W. R. CARPENTER

## Demonstrations of Affective Method

A recent article in the Journal mentions two demonstrations designed to accompany lectures on simple feeling, "a demonstration of the method of impression, by way of paired comparisons of colors, and a demonstration of the method of expression, by way of the pneumograph" (34, 1923, 471). The materials of these demonstrations are as follows.

*Method of Expression.* The kymograph has a light metal drum, 12 in. high and 12 in. in diam., which revolves (roughly) once in the 1 min. The construction is as simple as possible: the clock-house forms the base of the instrument, and the shaft of the drum stands free, without lateral support. The recording tambour is an exaggerated Marey tambour, about 5 in. across and 1 in. deep. The capsule (transmitting tambour) is of the Paul Bert type, home-made from a coffee-tin. The time-marker utilises the clock-work of a metronome. The two writing points are wound with wisps of cotton-wool, so that the curves drawn on the smoked paper are some eighth-in. wide.

The whole intent of the set-up is to demonstrate procedure; the audience watch the record of the assistant's breathing, and the writing of the time-line beneath it, and thus (one hopes) are prepared for the following discussion of method and exhibition of experimental records. If adjustments have been made beforehand, the demonstration runs off quickly and smoothly, without interruption of the lecture.

Kymograph, recording tambour and time-marker were built for me by the C. H. Stoelting Co. The tambour and time-marker can be simplified, and the demonstration in so far improved. The dimensions of the various pieces were chosen for an audience of two hundred.

*Method of Impression.* A sheet of neutral grey cardboard (dimensions will vary with circumstances) is fastened to a stout frame of blackened wood, which stands on a black wooden base. The cardboard has, at its center, two square windows of convenient size, set side by side. I will call this cardboard the 'screen.'

Behind the screen stands the 'first slide,' made of the same grey cardboard. The first slide come down just low enough to cover the windows in the screen. Above the windows it has an oblong cut-out, large enough to uncover the windows. Its top edge extends above the screen; the corners of the projecting edge are bevelled off.

Behind the first slide stands the 'second slide,' also of cardboard. It carries three pairs of color-squares, slightly larger than the windows of the screen, arranged pair above pair; the uppermost pair lies directly behind the windows, so that, if there were no first slide to cut them off, the colours would show through the windows.

As the apparatus stands, then, the audience see the two windows of the grey screen filled with the same grey (of the first slide). In demonstrating, the lecturer pushes down the first slide, against a spring, and so brings its cut-out behind the windows. The second slide stays in place, held by a ratchet behind; and the colors of the top pair appear in the windows. The lecturer removes his hand; the first slide is drawn up by the spring, till its grey again fills the windows; at the same time a ratchet on it lifts the second slide, and the middle pair of colors takes the place formerly occupied by the top pair.

The lecturer now pushes down the first slide a second time; the audience see the middle pair of colors in the windows. He removes his hand; the first slide comes up to fill the windows, and lifts the second slide with it as before; the bottom pair of colors is thus raised into the position originally occupied by the top pair.

The lecturer pushes down the first slide a third time, and the audience see the bottom pair of colors in the windows. He removes his hand, and the colors are replaced, finally, by grey.

The apparatus—which, in its latest form, is largely the work of Dr. Boring and Dr. Bishop—thus permits the serial exposure of three pairs of colors (in principle, of course, of more; but three are enough) by as many simple hand-movements. There are no loose colors to get dirty; the set-up is permanent. Also there is no loss of time for the lecturer.

E. B. T.

## The Sanford Foundation

Mrs. W. F. Frear of Honolulu proposes to give one thousand dollars to Wellesley College as nucleus of the Edmund Clark Sanford Foundation for Psychological Research. Mrs. Frear, who is a graduate of Wellesley, was a student in the school to which Mr. Sanford went as instructor immediately after leaving college. She makes the gift in grateful memory of his teaching and of his friendship. It is the more appropriate in that Dr. Sanford was keenly interested in the foundation, more than thirty years ago, of the Wellesley Psychology Laboratory. Some pieces of the original apparatus were made under his personal direction; and indeed he played, in those early days, the rôle of its kindly Providence. The friendly relation was never broken and one of the last letters which he wrote was to fix the date for a lecture at Wellesley. It is hoped that not a few of Dr. Sanford's friends may care to add to the endowment of which the accumulated interest will from time to time be used to purchase apparatus or to defray expenses of publication. Gifts or pledges may be sent to President E. F. Pendleton or to Professor M. W. Calkins, Wellesley College, Wellesley, Mass.

## Habitual Motivations

Stories of horses turning up a familiar lane, of the doctor's horse pulling up at the home of a former patient, of the milk-man's horse turning in at every house on the milk route, have often been told as evidence of 'memory' in animals. A different explanation of at least some of these incidents is suggested by the fact, which has recently come to my attention, that the automobile sometimes behaves in a similar way. Several physicians have told me that they find themselves, not infrequently, turning in, stopping, and even getting out with their medical cases at the house of some former patient. Since, unfortunately, none of my informants has kept a record of these occurrences, I cannot attempt an analysis. Abstraction, however, seems to be a necessary condition; for all the instances reported to me occurred when the physician, on his morning rounds, was tired or worried, and was driving automatically. It is reasonable to assume that, in some instances at least, the horse is similarly guided; and that, in both cases, abstraction plays the same rôle as it does in the common experience of catching oneself, when intending to change one's coat or tie, somewhere in the process of complete undressing.

K. M. D.